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FARTHER PROOFS

THAT

GLASS is permeable by the ELECTRIC EFFLUVIA;

AND THAT

The Electric Particles are poſſeſſed of a POLAR VIRTUE;

WITH

REMARKS on the MONTHLY REVIEWERS Animadverſions
on a late Work,

INTITULED,

"*Experiments and Obſervations made with a View to point out the Errors in the preſent received Theory of Electricity.*"

By the Reverend JOHN LYON, of DOVER, KENT.

K

> *Quel eſt notre but dans la phyſique rationnelle? Eſt-ce de nous contenter de mots? Contentons-nous en donc des l'entrée: reſtons aux qualités des anciens: cela ſera plus court, & tout auſſi raiſonnable.*
>
> J. A. De Luc's Hiſt. de la Terre & de l'Homme, Tom. I. p. 191.

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J. R. G. Aubrey, 1790.

TO

SIR JOHN HENNIKER, F. R. S.
OF NEWTON-HALL, IN THE COUNTY OF ESSEX;

AND

JOHN TREVANION, ESQ.
OF THE CITY OF LONDON, MERCHANT;

THE REPRESENTATIVES OF THE TOWN AND PORT OF DOVER:

THIS
TREATISE
OF
ELECTRICITY
IS,
WITH DUE RESPECT,
INSCRIBED,

BY THEIR MOST OBLIGED
AND OBEDIENT SERVANT,

THE AUTHOR.

TO

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THE AUTHOR.

30 JUN 59

ADVERTISEMENT.

AS ſeveral of the Experiments in the following Sheets, and particularly thoſe of lighting ſpirits of wine through the ſubſtance of glaſs, require great accuracy, and the ſucceſs depends upon accelerating the velocity of the ſpark; the practical electrician is requeſted not to reprobate either the experiments or their reſults, if he ſhould not ſucceed in a few of his firſt attempts. To accelerate the velocity of the ſpark, that it may overcome the attraction between it and the glaſs, and paſs through it in diſcharging the Leyden-phial, it is neceſſary to touch the knob of the jar with a very ſudden and quick motion, or the electric particles may, by the great attraction, lodge in the pores, and the glaſs be charged.

The Author aſſures the Reader, there is not a ſingle Experiment, either in his firſt Publication, or in the preſent Work, but what has, at different times, been repeatedly made; and ſome of them, reſpecting the ~~permeability of glaſs to the electric effluvia, have been~~ carefully and critically examined by ſeveral electricians, who were apparently unwilling to believe there could be ſo great an error in the preſent popular theory; but he has ſeen inſtances where rigid prejudice has relaxed its tenacious adherence to the Franklinian doctrine, and hardly doubted of its erroneouſneſs; and others, where candour has at once ſubſcribed to the truth of the permeability of glaſs to the electric fluid.

In a dark and intricate ſcience, like electricity, it is more than probable, that thoſe who firſt entered upon it have fallen into ſome errors, which have ſince been embraced by their ſucceſſors; and, as diſcoveries are daily making, have we not reaſon to expect that ſome falſe notions have got into our electric creed?

As the Author is ſenſible how liable the wiſeſt are to err, he is far from wiſhing his Polar Syſtem to be conſidered as a compleat one; he not only expects, but hopes it may be examined: and all he aſks, is a liberal and candid treatment from the real friends to truth. He alſo frankly acknowledges, that he not only expected, but waited for ſome of the Frankliniſts to object to his experiments and their reſults, that he might have an opportunity of replying; and, as the Monthly Reviewers have given him the opportunity he wiſhed for, he has taken it; but in future he never intends to take notice of any anonymous writer, who may think proper to oppoſe his doctrine.

ERRATA and ADDENDA.

Page	Line	*For*	*Read*
Page 38,	Line 1 and 2,	a knob to it,	a knob to each end of it.
ib.	3,	Place the end of the rod	Place the ball upon the end.
ib.	7,	point H	ball H.
ib.	20,	on the end H	on the ball on the end H.

A NOTE to EXPERIMENT V.

If the experimental electrician finds any difficulty in making the cone of rays appear between the outſide coating of the jar upon the ſtool and the knob on the end of the wire H, I adviſe him to ſlip the ſocket down the glaſs ſtand till he brings the knob H within three-eighths of an inch of the ſurface or edge of the ſtool; or, more expeditiouſly, if he only holds his finger near the edge of the ſtool, while the jar is charging, I make no doubt but he will ſee a cone of rays converging to his finger.

There will be alſo a bruſh or cone of rays converging to his finger, if a ſpark be taken from the knob of the jar by another perſon when charged.

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INTRO-

PLATE. I.

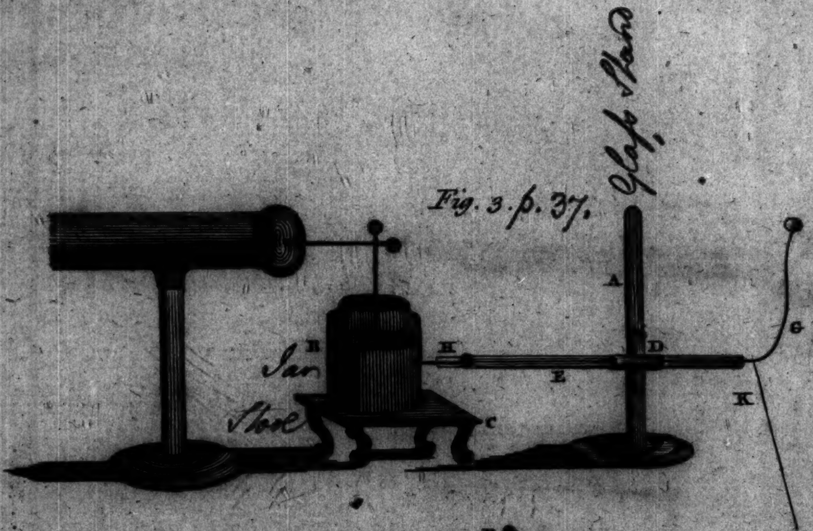

Fig. 3. p. 37.

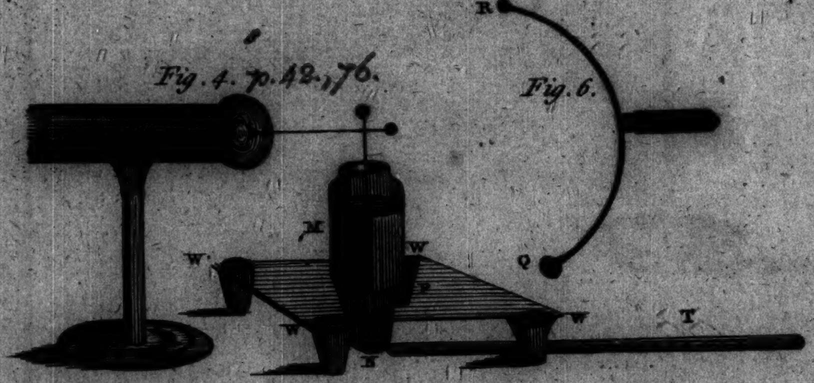

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Fig. 6.

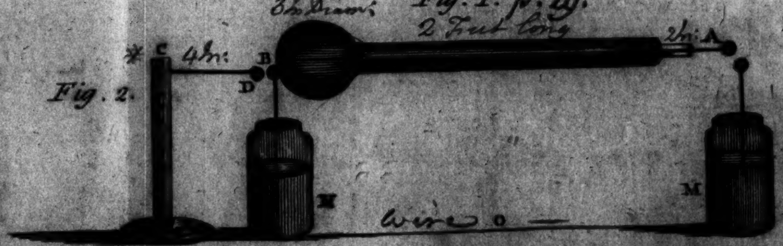

Fig. 1. p. 29.

Fig. 2.

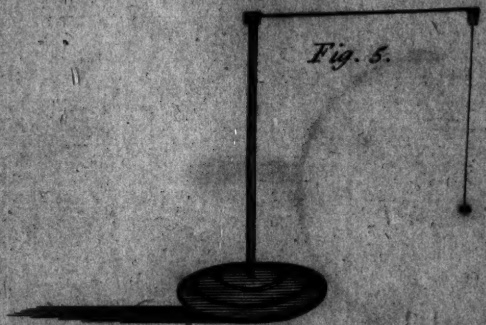

Fig. 5.

* the Seperation between the Tin foil $\frac{1}{24}$ of an inch –

INTRODUCTION.

WHEN I publiſhed my Experiments and Obſervations, with a view of pointing out the errors in the preſent received theory of Electricity, I was ſatisfied, from what I had previouſly experienced, that I ſhould meet with prejudiced and partial ſtrictures on my work, from zealous partizans of the Franklinian ſchool; who have many of them implicitly followed each other, and have taken it for granted, that their doctrine, like the laws of nature, ſtands upon an immoveable baſe.

There have been ſome few perſons, at different intervals, who have diſſented from the popular theory; but not being able to produce ſufficient proof to counteract prejudice, they have either gone with the current, or enjoyed that opinion in private, which they could not publickly ſupport.

This has encouraged thoſe, who have adopted the opinions and ſyſtems of others, without proving them by the ſlow and laborious method of making experiments, and comparing their reſults with the ſimple operations of nature, to ſtigmatize with their marks of diſapprobation, every doctrine, though unexamined, which is likely to ſet inquiſitive men upon thinking for themſelves. Such perſons, in the midſt of their zeal, frequently miſtake received hypotheſes for eſtabliſhed truths; and, if I am not much deceived, the Monthly Reviewers of the philoſophical department, with all their learning and experience, have lately fallen into this ſnare. They have publickly accuſed me of having publiſhed a work "to perplex the Frankliniſts with a ſet of "electric ænigmas," and conſequently, as they think, to retard the progreſs of real ſcience; but they have not given a ſingle experiment, or a ſatisfactory remark, to ſupport what they have ſaid.

I would candidly acknowledge myſelf deſerving of cenſure, and retire at once from a controverſy, which I have reaſon to believe, from what I have experienced, will not be conducted, on the part of my opponents, in a liberal manner, if I could perſuade myſelf that what I have offered, and am about to offer, could be placed beyond the reach of prejudice to controvert.

As I am not conſcious that I have either been prejudiced in my inquiries, or influenced by any mean or illiberal motive, I am not afraid of meeting any attack from that quarter; and, ſince my labours have been tried and condemned in the court of criticiſm eſtabliſhed by the Monthly Reviewers, I think proper for once to appeal to a higher authority, and to plead my cauſe with facts at the bar of the Public; where I hope and truſt I ſhall have a candid hearing.

The method I am obliged to purſue in my defence, is traced out for me by my adverſaries; and, to be as clear and as intelligible as I can to my readers, I ſhall number each clauſe of my ſentence of condemnation, and plead to them diſtinctly, before I produce my experiments, as a further proof of what I have aſſerted in my firſt work.

I.

In this field of cenſure, upon which I am now entering, and where truth has often bled beneath the ſting of the critic, and languiſhed in ſilence for want of a friendly ſupport, I find the following introductory common-place preface: "When a writer "has evidently taken conſiderable pains to inveſtigate a particular ſubject, by a great variety of experiments, and who "has, in general, expreſſed their reſults without arrogance, "fails in his purpoſe of informing the world, we feel a ſincere "pain in diſcharging our duty to the public, by pointing out "his defects. That duty, however, muſt be performed by us "on the preſent occaſion; not as partizans of Dr. Franklin, "but as friends to truth, and well-wiſhers to the progreſs of "real ſcience [1]."

All I have to remark under this ſection is, that I am apprehenſive I have been thought wanting, in neither preſenting incenſe or a peace-offering at the altar of criticiſm, as practiſed by ſome authors, as a powerful antidote againſt the cenſures of the critic. But, as I pay no regard to praiſe unmerited, nor fear cenſure undeſerved, I never did, nor will, take any indirect method to procure the one or to avoid the other.

II.

If a dictatorial method of deciding upon any ſubject in diſpute has a tendency to ſtop or retard the progreſs of ſcience, the following ſentence appears to me to be calculated for this purpoſe:

"If the Franklinian ſyſtem of electricity is to be overturned,

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[1] Monthly Review, January 1781, p. 1.

" its demolition muſt be effected by means very different from " thoſe employed by the preſent author *."

I cannot paſs this ſingular mode of deciding upon a ſubject, without remarking, that the method which I have purſued, or ſhould have purſued, ought to have been pointed out to the public by the Monthly Reviewers, or their opinions ought to have been expreſſed in a leſs confident manner. For want of this, they have made themſelves both judge and jury, and paſſed ſentence of condemnation without condeſcending to aſſign a ſingle reaſon in proof of their aſſertion. As I ſcorn to ſculk behind any mean ſubterfuge, or trifling evaſion, I ſhall briefly relate the plan I have purſued in attacking the Franklinian doctrine, that the world may form a judgment, whether the Reviewers have acted " as friends to truth, and well-wiſhers to the " progreſs of real ſcience." When I at firſt applied myſelf in earneſt to examine the ſeveral electric phænomena, I began with conſidering every effect muſt have a cauſe, and that the cauſe, when a natural one, muſt alſo be a ſimple one: for nature does nothing in vain; and more is vain when leſs will ſerve.

I next examined the different branches of corpuſcular attraction and repulſion, to try if I could diſcover any ſimilar effects to thoſe produced by electricity; and I ſaw, or thought I ſaw, a very ſtriking reſemblance, in many inſtances, between the electric and the magnetic effluvia, in their attracting and repelling each other.

As magnetic attraction and repulſion is ſuppoſed to be owing to a polar virtue in the magnetic particles, I apprehended there might be a polar virtue in the primoriginal particles of other bodies; I therefore examined the diſſolution of bodies,

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* Monthly Review for January 1781, p. 1.

bodies, and cryſtalization of ſalts, to try if theſe operations of nature would furniſh me with any farther proof. From the experiments which I made, I could not draw any other concluſion than this; *viz.* That the corpuſcular parts of ſubſtances do attract and repel, at ſmall diſtances; and that this attraction and repulſion probably is owing to a polar virtue in the firſt particles of matter.

Though this was, and ſtill is, my private opinion, I did not venture to build any hypotheſis upon it, till I had firſt conſulted Sir Iſaac [3] Newton's Principia, and found myſelf authorized in reaſoning from my experiment and obſervations; and from the analogy I diſcovered between them and the different operations of nature; and that propoſitions collected from general induction from phænomena, were to be conſidered as nearly true, notwithſtanding any contrary hypotheſis that may be imagined, till ſuch time as other phænomena occur, by which they may either be made more accurate, or liable to exception.

Under the ſanction of ſo great a maſter, I ventured to attack the Franklinian doctrine; not without conſidering, that if I attempted to raiſe a new ſtructure where the old building is ſtanding, I muſt firſt clear away the rubbiſh which encumbered the place.

In order to effect this, I began with the two principal properties which electricians have aſcribed to electrics.

The 1ſt. They are inſulating; *viz.* of ſuch a nature as not to let the electric particles paſs over their ſurfaces.

The 2d. They are impenetrable, and do not admit the electric particles through their pores.

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[3] Motte's Newton's Principia, vol. ii. p. 203.

I have proved, and I believe beyond the reach of controverſy[4], that the electric fluid does paſs over the ſurface of glaſs, when it cannot go off another way.

The Monthly Reviewers[5] are inclined to admit the truth of my firſt and ſecond experiments; therefore they have given up the firſt property, which Pere Beccaria has aſcribed to an electric. Though they have patiently parted with the firſt property aſcribed to glaſs, they are determined, at all events, to maintain the ſecond; *viz.* That an electric is impermeable to the electric fluid.

It is acknowledged by the Reviewers, "If there were no error in the relation of what is offered under this head, it would "afford a moſt deciſive proof againſt the ſuppoſed impermea-"bility of glaſs, at leaſt with reſpect to an electric charge; and "would, in fact, ſhake the Franklinian ſyſtem to the very foun-"dation[6]."

Inſtead of their pointing out the real, or the ſuppoſed error, as they ought to have done, they have taken the moſt expeditious method of getting rid of a difficulty, by indirectly denying the[7] truth of the

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[4] See Experiments and Obſervations, made with a view to point out the errors, &c. p. 15 and 16.

[5] Monthly Review for January 1781, p. 4.——It is well known, that when a vial is highly charged, and the electrization continued, and vigorous, the electric fluid will paſs over the ſurface of glaſs, and other electrics, how dry ſoever, to a conſiderable diſtance. The author himſelf, in the ſecond experiment related in this work, ſhews that it will paſs to the diſtance of four feet.

[6] Monthly Review for January 1781, p. 2.

[7] Idem.——Though we might content ourſelves with giving this experiment, as here related, a flat contradiction, &c. &c.

We entertain not the leaſt doubt of Mr. Lyon's veracity; but ſuppoſe that he may the reſult of my experiments, and then ſuppoſing I have been deceived.

As the permeability of glaſs to the electric fluid is at preſent a diſputed point, I ſhall not pretend to ſay here I have abſolutely proved it, till I have offered ſeveral other corroborating facts, in ſupport of what has been ſo illiberally and poſitively reprobated by the Monthly Reviewers.

Their treatment, however harſh, did not in any degree diſcompoſe my temper; and, as they expreſſed a deſire of a farther explanation to my ſixth, or, as they term it, "ſingular experiment," I addreſſed the following letter to their Editor for them; and, as they thought proper to publiſh only an extract, I ſhall lay the whole of it before the Public; and they will then have no difficulty to determine why a part of it has been ſuppreſſed.

"To the Editor of the Monthly Review.

"Gentlemen,

"On peruſing your animadverſions on my publication, in your laſt Review, I was rather ſurprized at your intimating, that you ſhall take a pleaſure in communicating to the Public any thing more relative to my ſixth experiment; eſpecially as you conceive my prejudices [a] ſo great as to render me incapable of judging of truth when it is before me.

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may have been deceived, and that the electric matter may have paſſed over the ſurface of the glaſs. If, however, he will tranſmit to our Editor any farther explanation of this ſingular experiment, we ſhall take a pleaſure in communicating his obſervations to the Public in our correſpondence.

[a] Monthly Review for Jan. 1781, p. 7.——His inquiries in this branch of philoſophy would undoubtedly have been more ſucceſsful, had he not evidently proſecuted many of them under the influence of prejudice.

"What our opinions of each other may be, is of little conſequence in the preſent diſpute; my purſuit is TRUTH; and to its unerring teſt I am ready at all times to ſubmit, whatever I have, or may in future, offer to the Public on this ſubject.

"To obviate, as far as I am able, all obſcurity, I will endeavour, with brevity and clearneſs, to deſcribe the neceſſary preparations for performing this experiment with ſucceſs [9].

"Take a pane of crown-glaſs of any dimenſions (even as far as four feet ſquare, or as many in diameter, if round); let it be dry, warm, and clean; place it flat upon the table, with a common-ſized jar upon the center of it, and the knob of the jar in contact with the prime conductor. Put one end of the chain under the center of the glaſs next the table, and (to avoid the trouble of fixing the points, as mentioned in p. 20 of my work) place one end of a narrow ſlip of tin-foil upon the upper ſurface of the pane oppoſite to the chain, and bend the other part of it to the outſide coating of the jar. Take the other end of the chain in your left hand; then, after a few turns of the cylinder (if the apparatus be in proper order) ſtrike the conductor with your right hand with a very ſudden motion; and, if you do not hear the exploſion, ſee the ſpark, and feel the ſhock in both arms, though the ſuppoſed impenetrable barrier lie at the bottom of the jar, I ſhall only ſay, my ſenſations of hearing, ſeeing, and feeling, are very different from yours.

"You are apprehenſive, and therefore have taken it for granted, my prejudices have bewildered and deceived me in this caſe; as a proof that your charge, though ſevere, is not founded in truth, I ſhall readily give you my reaſons why I think I am not bewildered; and I ſhall be as willing to retract my opinion, as I was to adopt it, the moment I am convinced it was founded in error. When the ſhock is taken through the glaſs, and without perforating it (for I doubt not in the leaſt but it may be perforated with a large jar charged very high) the noiſe of the exploſion is quite different, the velocity of the ſpark is checked, neither is the ſenſation near ſo pungent as in the common diſcharge.

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[9] Sixth experiment, p. 20. of Experiments and Obſervations, &c. by J. Lyon.

"If the atmoſphere be favourable for electrical experiments, and every part of the apparatus in proper order, I hardly ever fail of giving the ſhock with the pane, as before directed, at the bottom of the jar.

"I have tried this experiment a great many times; and, ſince your animadverſions, I have repeatedly taken the ſhock, as have ſeveral gentlemen, who are as well ſatisfied as myſelf, that the ſpark paſſed through them, and through the pane.

"When I firſt made a trial of this kind, I did not ſucceed, neither in my firſt, my ſecond, nor my third attempt; yet this was not owing to the ſuppoſed impenetrability of the glaſs to the electric fluid, but to my not knowing how to conduct the experiment. I do aſſure you, I have not truſted to one experiment frequently repeated; I have different methods of conveying the ſhock through glaſs, if this ſhould be objected to by electricians.

"I have a method, of my own invention, to cauſe the electric ſpark to mark its courſe over a body; and it may probably afford me a clear and deciſive proof, that electricians have been deceived in the ſuppoſed directions of the ſtar and the bruſh.

"I have a variety of new experiments, both in magnetiſm and electricity, to ſupport what I have only hinted in my publication;

tion; but what I can do, or how I do it, the ſcanty limits of your paper will not permit me to enlarge on.

"If I ſhould happen to meet with a very moderate degree of encouragement from the Public, and a little liberal treatment from the real friends of ſcience, I may probably be induced, in due time, to publiſh my farther experiments, remarks, and obſervations on theſe ſubjects. You have recommended to my peruſal, Mr. Wilſon's "Short View of Electricity;" I can aſſure you I have read it with pleaſure; and I ſubſcribe to the truth of what he aſſerts, p. 20; *viz.* "That more time is required for the "electric fluid to paſs through a given length or thickneſs of "glaſs, than through metal or wood of the ſame length."

I am, Gentlemen,

Yours, &c.

Dover,
7th Feb. 1781.

JOHN LYON."

By ſuppreſſing the beginning and the concluſion of my letter, it is clear to me, that my adverſaries are as ready in caſting a veil over their own ſlips and infirmities, as they are in expoſing thoſe of others; but ſuch trifling evaſions can never be ſerviceable in any cauſe. If they have recommended to my notice an author, who, as well as myſelf, maintains the permeability of glaſs to the electric fluid, by what juſtifiable motive could they be induced to omit mentioning the coincidence of this Gentleman's opinion with mine upon this ſubject? I am ſo far from wiſhing to hide any weak part, or to fly from any difficulty, that I ſhall cooly and deliberately meet (as my opponents think) their unanſwerable remark, which, I am apprehenſive, ſtands upon a very tottering foundation.

III. They

III.

They are pleaſed to ſay, " I have [10] undertaken to ſhew how " a veſſel becomes the better adapted to receive and contain a " fluid poured into it, becauſe it is full of holes, which permit " juſt as much of the fluid to run out, or through its ſides, as " is poured into it; whereas if the veſſel were perfectly ſound, " or impenetrable, it would neither receive or retain a drop " of it:—And his ſucceſs is ſuch as might be expected in ſo " hopeful an enterprize."

If this remark be not the offspring of prejudice, I confeſs myſelf at a loſs to know from what other ſource its origin can be traced.

I deny that I have any where ſaid, " That a veſſel becomes " the better adapted to receive and contain a fluid poured into " it, becauſe it is full of holes, which permit juſt as much " of the fluid to run out, or through its ſides, as is poured " into it." Every perſon knows, if a fluid runs out as faſt as it is poured into a veſſel, that veſſel cannot fill.

If I have aſſerted, that the electric fluid paſſes through the pores of glaſs, I am far from wiſhing to retract my aſſertion; I am ſo far from it, that I even flatter myſelf of returning from this forlorn hope with victory.

I am ſenſible electricians could never conceive how the Leyden-phial could charge, if the electric fluid could find a paſſage through the pores of the glaſs. This difficulty has been a ſtumbling-block to all the practitioners in this branch of natural philoſophy, ever ſince the time Profeſſor Muſchenbroek accidentally received the ſhock, to this preſent minute; and

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[10] Monthly Review for January 1781, p. 5.

merely for want of examining the common operations of art and nature, making uſe of their own faculties, and reaſoning from analogy; which appears to be the moſt certain method, in all our natural enquiries, of arriving at truth; which will neither admit of a mathematical or geometrical demonſtration.

That a veſſel may be filled with a fluid, when full of pores; and that a porous body may be ſaturated with a fluid; are facts ſo well known to almoſt every one, that I ſhould think them too trifling to mention here, if the Monthly Reviewers had not intimated, that by my maintaining the permeability of glaſs, I have undertaken to ſupport a natural impoſſibility.

To convince thoſe, who pay a greater regard to the truth of a propoſition, than to the quality, or eminence of its authors, that my prejudices have not carried me to the fartheſt limits of abſurdity, I ſhall produce a very common and ſimple experiment, known to every one who has been accuſtomed to the filtering of liquids.

Take a piece of fine flannel, or woollen cloth, and cut it in ſuch a manner that the ſides, when ſewed together, may bring it to the form of a hollow cone. Fix the baſe, or wideſt part of this cone, round a hoop of the ſame dimenſions, and ſuſpend it with a cord from a croſs or pin, that it may hang clear of any other body, and it will form a kind of funnel, with the point of it downwards: or, what the faculty call Hippocrates' Sleeve. I ſhall take it for granted, it will be allowed me this ſleeve is porous, and will permit a fluid to paſs through its ſides; yet if any liquid, I will not except the moſt pure ſpirits, be poured gently into it; the ſleeve may be filled without any difficulty, though the liquid be diſtilling through every pore.

Let a cup be hollowed out of a large filtering-ſtone, and the ſides of it cut as thin as the ſubſtance of the ſtone will admit, without breaking it. Dip the cup in water, and ſaturate the ſtone with the fluid. Take it out, and pour water into the cup; and will any perſon undertake to ſay the cup cannot be filled, when the fluid is finding a paſſage through every pore in the bottom and ſides of the cup?

The cells of bread, ſugar, and a variety of other ſubſtances, imbibe fluids plentifully; and there are ſeveral bodies whoſe pores are ſo exceedingly minute as not only to imbibe but even to ſaturate the cells with the electric fluid. The retainers of Sir Iſaac Newton's Philoſophy, ſolve the aſcent and the deſcent of liquids in the filaments or threads of cloth by attraction [11]; and electricians themſelves acknowledge, there is a ſtrong attraction between the electric fluid and glaſs [12].

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[11] Clare's Motion of Fluids, p. 32.——The ſame cauſe (*viz.* attraction) which inclines fluids to riſe in ſmall tubes above the level of the reſt, which they very notably do, makes them deſcend alſo in the filaments or threads of cloth, in the way of filtration, which ſooner takes effect if the pores of the cloth be firſt filled with liquor. The cells of bread, ſugar, and other porous ſubſtances, for a like reaſon, imbibe fluids plentifully, in which they riſe againſt the direction of gravity, for the reaſon juſt aſſigned.

[12] Dr. Franklin's Experiments and Obſervations, p. 75.——That this electrical fluid or fire is ſtrongly attracted by glaſs, we know from the quickneſs and violence with which it is reſumed by the part that had been deprived of it, when there is an opportunity—and by this, that we cannot, from a maſs of glaſs, draw a quantity of electric fire, or electrify the whole maſs MINUS, as we can a maſs of metal. We cannot leſſen or encreaſe its whole quantity; for the quantity it has it holds; and it has as much as it can hold. Its pores are filled with it as full as the mutual repellency of the particles will admit; and what is already in refuſes, or ſtrongly repels, any additional quantity. Nor have we any way of moving the electrical fluid but one; that is, by covering part of the two

ſurfaces

In the foregoing note, which I have extracted from Dr. Franklin's works, it is allowed that glaſs has pores; that theſe pores are as full of the electric fluid as the mutual repellency of the particles will admit; and that the particles already contained in the pores of the glaſs refuſe, or ſtrongly repel, any additional quantity.

If the electric effluvia contained in the pores of the glaſs have a repelling property, ſufficient to counteract the fluid condenſed on the upper ſurface of a pane of glaſs, or the inſide of a jar, ſo as to prevent the electric particles thrown into the jar, or upon the ſurface of the pane, from entering the pores; it is to me an inexplicable myſtery, by what means they can repel the electric effluvia out of the pores on the oppoſite ſurface of a pane of glaſs. To a mind free from prejudice and prepoſſeſſion, it will appear as great a natural impoſſibility, as it is an abſurdity to ſuppoſe it.

And yet, abſurd as this doctrine appears to be, it has its followers; and it is ſo poſitively maintained, that a perſon can hardly fail of incurring cenſure if he oppoſes it.

The Franklinian hypotheſis is neither leſs nor more than a mixture of the Newtonian philoſophy of attraction and repulſion, and of the Peripatetic; *viz.* of occult qualities; of bodies acting upon other bodies, by ſome unknown property, through an impenetrable barrier: but we ſhall find, upon a careful examination,

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ſurfaces of this glaſs with non-electrics, and then throwing an additional quantity on one ſurface, which ſpreading in the non-electric, and being bound by it to that ſurface, acts by its repelling on the particles of the electrical fluid contained in the other ſurface, and drives them out of the glaſs into the non-electric, on that ſide from whence they were diſcharged, and then thoſe added on the charged ſide can enter.

mination, the two ſyſtems of theſe maſters will no more incorporate with each other than water and oil.

I ſhall be very happy to be informed, by ſome rational inquirer after truth, why the particles of the electric fluid cannot paſs through the pores of glaſs? It cannot be from the minuteneſs of the pores, nor the ſize of the particles; becauſe it is granted the pores are filled with the electric effluvia, and that on both ſurfaces of the glaſs.

I am ready to allow, with the Frankliniſts, that there is a ſtrong attraction between the electric fluid and glaſs; but I believe that this attraction ariſes from a very different cauſe from what has hitherto been aſſigned by any of them. I make not the leaſt doubt, but that thoſe who are acquainted with the laws of attraction, in the different branches of natural philoſophy, already comprehend my meaning; but for the ſake of thoſe who may not be ſo well verſed in the attraction of fluids in capillary tubes, I ſhall endeavour to be more explicit.

Glaſs is perhaps of all other ſubſtances, which either art or nature has produced (diamonds excepted) the moſt ſolid. This is evident, by breaking with a ſmooth and a poliſhed edge, when moſt other ſolid bodies, and even gold itſelf, breaks with a rough ſurface. From this circumſtance, as well as from well-ſtopped phials keeping the moſt ſubtile and piercing menſtruums, whether acid, urinous, or volatile, it muſt be allowed that the pores of glaſs are exceedingly ſmall, ſo minute as to prevent any fluid, excepting the primoriginal particles of matter, from entering them. It is the ſmallneſs of the pores which cauſes the attraction between the electric fluid and glaſs.

The interſtices of all bodies may be conſidered as capillary ducts, which attract fluids in a reciprocal proportion to the diameter of the cavity of their pipes [13].

Though ſpunge, aſhes, ſugar, and many other bodies [14], attract fluids, they do not act ſo forcibly as glaſs. As the aſcent of fluids in theſe different kinds of ſubſtances is from one and the ſame principle, *viz.* an attraction between theſe ſeveral ſubſtances and the fluid, if we reaſon from analogy, we muſt conclude, that the attraction between the electric effluvia and glaſs is owing to the particles entering the pores of it.

It is very probable, that the pores of the glaſs attract the electric fluid, as capillary tubes do liquids; *viz.* in a reciprocal proportion to the diameter of their cavity. I once had a box of jars from the glaſs-houſe, which I never could charge, owing, as I apprehend (for I know of no other deficiency) to the pores of the glaſs being too large to attract the electric effluvia, with a force ſufficient to overcome their velocity, in running from the conductor through the ſubſtance of the glaſs.

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[13] Newton's Optics, p. 336.——If ſlender pipes of glaſs be dipped at one end into ſtagnating water, the water will riſe up within the pipe, and the height to which it riſes will be reciprocally proportional to the diameter of the cavity of the pipe.

[14] Sir Iſaac Newton's Optics, p. 367.——If a large pipe of glaſs be filled with aſhes, well preſſed together in the glaſs, and one end of the pipe be dipped into ſtagnating water, the water will riſe up ſlowly in the aſhes, ſo as in the ſpace of a week or a fortnight to reach up within the glaſs to the height of 30 or 40 inches above the ſtagnating waters. The water riſes up to this height by the action only of thoſe particles of the aſhes which are upon the ſurface of the elevated water; the particles which are within the water attracting or repelling it as much downwards as upwards: and therefore the action of the aſhes is very ſtrong. But the particles of the aſhes being not ſo denſe and cloſe together as thoſe of glaſs, their action is not ſo ſtrong as that of glaſs, which keeps quickſilver ſuſpended to the height of 60 or 70 inches, and therefore acts with a force which would keep water ſuſpended at the height of above 60 feet.

By the ſame principle a ſpunge ſucks in water, and the glands in the bodies of animals, according to their ſeveral natures and diſpoſitions, ſuck in various juices from the blood.

But we are told by Dr. Franklin, that the pores of the glaſs are pre-occupied by the electric particles [15]; that they are as full as the mutual repellency of the effluvia will admit; and what they have they hold; and what is contained in the pores of the glaſs ſtrongly repels any additional quantity.

As all bodies have a great many more pores than ſolid parts; according to this doctrine there muſt be a large portion of the ſurface of the glaſs reſiſting the charging of it; and the attraction between the electrical particles and the glaſs can only be with the ſolid parts of the ſurface of the pane or jar.

It is further worthy a remark, that, according to this hypotheſis, the electrical particles in the pores of the glaſs, by repelling thoſe which are ſuperinduced, muſt keep their ſtation; and there can be no rational reaſon aſſigned, why they ſhould repel the particles from the under ſide of the pane.

I muſt leave it to thoſe, who are much deeper read in corpuſcular attraction and repulſion than myſelf, to ſhew how this attraction between the ſolid parts of the glaſs and the electric fluid, and the double repulſion in the pores of the glaſs, can take place at the ſame time.

Dr. Franklin evidently ſaw (and, in my opinion, much clearer than his followers) a difficulty in this doctrine, and he endeavoured to remove it; but, inſtead of removing the firſt obſtacle, he has thrown another in the way. He ſays the non-electric [16],

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[15] See Note [12], p. 13, foregoing.

[16] Ibid.

viz. the tin-foil, being ſpread on one ſurface of the glaſs, it binds the electric effluvia to the glaſs; and the electric effluvia, thus bound, act by their repelling power on the particles inherent in the ſubſtance of a pane or jar, and they repel them to the tin-foil on the oppoſite ſurface, and the tin-foil diſcharges them.

Is not this making the tin-foil, like the glaſs, poſſeſs two different properties? In the firſt inſtance, by ſome occult quality, it retains the electric effluvia ſuperinduced; but, in the ſecond, it leads off or diſcharges the effluvia from the ſurface of the pane.

I flatter myſelf, that every unprejudiced perſon, who has accuſtomed himſelf to examine the nature and properties of different ſubſtances, will agree with me, that ſuch oppoſite qualities as this doctrine aſcribes to metal and glaſs, when connected, cannot be conſiſtent with thoſe ſimple and uniform principles which the Almighty has impreſſed upon all matter, and by which it invariably acts.

As is is granted the electric particles can enter the pores of glaſs, I ſhall not ſcruple to ſay, that I think, when ſuch ſmall bodies as the electric effluvia impinge[17] on the ſurface of glaſs, they are abſorbed by it, and the pores, being ſo very minute, the attraction is ſufficient to retard the particles in their paſſage through the pores of the glaſs.

If the ſubſtance[18] of the glaſs be thick, then the attraction is ſo great between the electric particles and the glaſs, that there is no giving the electric fluid a ſufficient velocity to overcome the attraction; and as ſoon as the particles are ſtopped, they become repulſive, and the repulſion is ſufficient to ſtop the charging of the jar. The ſame will happen to a thin piece of glaſs, if there be no conducting ſubſtance on the oppoſite ſide, to weaken or counteract the attraction between the electric particles and the glaſs. This attraction between glaſs and the electric effluvia being much ſtronger than the gravitation of the electric fluid, the particles cannot run together in drops, like the groſſer fluids upon the ſurface of a pane; and this, and this only, appears to me to be the true and rational cauſe why glaſs will not charge, when there is no coating to lead off the electric fluid while it is condenſing in the jar.

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[17] See Experiments and Obſervations on Electricity, &c. p. 199; or Newton's Optics, p. 241.

[18] See Experiments and Obſervations as above, p. 52 and 53, Experiments 9 and 10.

As I have now replied to their ſtrictures, ſo far as they regard the theoretical part of my work, it will next be neceſſary to attend to their remarks upon the practical part of it.

IV.

"[19] The following experiment is produced by the author, with "a view to ſhew the inſufficiency of the Franklinian theory to "account for the principle upon which the Leyden phial acts. "In our apprehenſion, it only ſhews, that the author has not "ſufficiently ſtudied the theory which he attempts to demoliſh, "without ſubſtituting a better, or any other in its room."

The experiment alluded to in the above extract, is experiment the ſixth, p. 44, of my Obſervations, on the charging and diſcharging the Leyden phial. It is a variation of the firſt experiment of the ſixth chapter; and I can ſay farther, it is the only one of that claſs to expoſe, in their opinion, the weakneſs of my attempt, and to turn, as they think, my artillery againſt me: but I ſhall by no means part with it, till I am ſubdued by demonſtrable facts, or plauſible arguments; for I can yet play it upon them with ſome effect.

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[19] Monthly Review for January 1781, p. 3.

As I cannot reconcile myſelf to reprinting the experiment here, I muſt refer my reader to it; and ſhall immediately attend to their remarks: "[20] It is ſufficient," they ſay "to aſk the author, how it is poſſible, ſuppoſing both jars to be placed upon the table, or their outſides to communicate with the earth, that the ſecond jar, or that, to the outſide of which the diſcharging-rod is not applied, can fail of being diſcharged, when, at the time of the diſcharge, the diſcharging-rod forms a communication between its inſide coating and the earth? It would be ſtrange, indeed, if the ſecond jar were not diſcharged," in this ſituation.

If theſe Monthly critics were but as well diſpoſed to candid examination as to cenſure, they would not have aſked me any ſuch queſtion; for I had previouſly declared my whole apparatus to be inſulated[21], and that no part of it had any communication with the floor. This they have omitted to take notice of, that their criticiſm might appear plauſible; and they have concluded their ſection, ſaying, "were the jars inſulated, a complete diſcharge would not take place."

As my table was inſulated with glaſs legs, they muſt know the jars ſtanding upon it could not communicate with the floor; and yet I dare affirm, and that againſt their poſitive aſſertion, a complete diſcharge will take place: and this may ſerve to ſhew I have attended to the Franklinian ſyſtem, and ſtudied it more carefully than they appear to wiſh the world to believe I have.

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[20] See Monthly Review for January 1781, p. 4.

[21] See Experiments and Obſervations, &c. p. 39.——There is as perfect an inſulation of every thing connected with the machine, as glaſs can form.

As they have thought proper to cavil at this fourth experiment, and to omit the other nine in this chapter, ſome of which would have ſpared them the trouble of their remarks; I ſhall ſpeak to the point in diſpute, with an experiment to ſhew the table is not a proper conductor to convey the electric fluid with a velocity ſufficient to give the ſhock.

EXPERIMENT *with two jars, charged at different conductors, to ſhew they will not give the ſhock, unleſs there be a better conducting body than the table between their outſide ſurfaces.*

Place a jar at each conductor, with a chain or a wire reaching from the outſide coating of one to the outſide coating of the other, and charge them. According to the Frankliniſts, the jar at the conductor next the cylinder will be full, and ready to throw off; the jar at the conductor ſtanding at the cuſhion empty, and extremely hungry: take both the jars from the conductors, and place them upon the table, about eighteen or twenty inches from each other, and without any metallic ſubſtance between them: with a diſcharger form a communication between the two knobs; if a part of their charges run off, the jars will neither give a ſhock nor any exploſion. If a wire be placed between the coatings of the jars, and a communication formed between their knobs, the diſcharge will be compleated with an exploſion, and the ſhock may be received.

I have repeatedly tried to diſcharge a ſingle jar with a ſpark, and a ſhock, after placing it ſix or ſeven inches from the edge of a table, and forming a communication between the ſide of the table neareſt to the jar and the knob of it; but if the charge would run off, it would be in a ſtream, and not with the velocity it does when there is a direct communication between the inſide and outſide of the jar, with the conducting bow.

From this I conclude, it would have been as much to the credit of the Reviewers to have kept their remarks on my fourth experiment, p. 44, to themſelves, and to have ſtudied attentively the work they recommended to my notice; and they would have learned, "[22] that more time is required for the elec-
" tric fluid to paſs through a given length or thickneſs of wood,
" than through metals of the ſame length;" therefore it is not ſo ſtrange as they may think, when I ſuppoſed, that, according to the Franklinian doctrine, the equilibrium could not be inſtantaneouſly reſtored in the ſecond jar.

V.

We are now to paſs on from p. 44 to p. 108, of my publication, where begins the tenth chapter; and here I find theſe critical gentlemen very confident in their aſſertions, and but little attentive to produce any proof of the error of my opinion.

"[23] We have more than once ſtudied (this chapter) from be-
" ginning to end, without being able to diſcover in it any thing
" that carries in it the moſt diſtant appearance of an analyſis.
" We here meet indeed with ſeveral experiments, which, ſo far

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[22] See Wilſon's Short View of Electricity, p. 20.

[23] Monthly Review for January 1781, p. 4.

"as we underſtand them, do not tend to throw the leaſt "additional light on the Leyden phial: much leſs are they "adapted to overturn Dr. Franklin's ſimple and luminous ex- "planation of its ſingular properties."

I ſhall not do juſtice to myſelf, if I paſs this remark without aſking my antagoniſts, Whether my tenth chapter is deſigned to overturn Dr. Franklin's ſimple and luminous explanation? and whether I have not adapted the ſeventh chapter entirely for that purpoſe? I flatter myſelf I have there ſhewn the ſtar is no infallible ſign of the fire entering a point; neither is the bruſh any ſure indication of the electric fluid iſſuing from a body. As ſome of theſe experiments, as well as experiment the eighth, p. 154, cannot be explained away, it was thought neceſſary to paſs over them in ſilence; as it is much eaſier to cavil at certain paſſages, than to controvert a ſeries of facts.

I believe I have proved, as clear as experiments can do it, that what electricians have called the reſiduum of a charge, ariſes from a very different cauſe than what they ſuſpected, and that my tenth chapter does throw ſome light on the properties of this wonderful jar. As I intend, in the following ſheets, to produce a variety of new experiments, as a ſupplement to my tenth chapter, and ſixth experiment of the ſecond chapter of my work, I ſhall reſerve my farther obſervations on the foregoing remarks of the Reviewers, at the head of this ſection, to be inſerted under my experiments; and I flatter myſelf I ſhall there ſhew, to thoſe who like to be at the trouble and expence of following me in my purſuit, that my labours have not all been in vain.

VI.

[24] “The author indeed here, as well as elſewhere, carries on an “ analogy between electricity and magnetiſm, as others have done “ before him, and aſcribes a kind of polar virtue to the par- “ ticles of the electric fluid.” I am ſenſible that ſeveral electricians have amuſed themſelves with ſtriking needles in different directions, to give them an attractive virtue; but I am totally unacquainted with the authors I have copied or followed in this part of my work. I aſſert, and with truth, that I am not beholden to any one for my plan, in comparing magnetic and electric attraction and repulſion with each other; but I do not undertake to ſay, there is not any thing of the kind publiſhed: if there be, it has not reached my hands, nor am I guilty of plagiariſm, as they wiſh to inſinuate by the above remark. In all caſes where I recollected that I had received information from the labours of others, I have been, and ſhall ſtill continue to be, ready to acknowledge it.

VII.

I am accuſed by the Reviewers, of having attempted to demoliſh the Franklinian hypotheſis, “ without ſubſtituting a “ better, or any other in its room [25]; yet, p. 5, my antagoniſts “ ſpeak of the author's POLAR SYSTEM; and they hint it “ might not be difficult to frame a plauſible hypotheſis, on “ the ſuppoſition that the electric particles are endued with “ poles.”

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[24] Monthly Review for January 1781, p. 5.

[25] Ibid. p. 3.

Surely

Every perſon, of a liberal turn of mind, will, I think, treat ſuch evaſive and contradictory remarks, with the contempt they deſerve.

I cannot follow my adverſaries from the tenth, to the thirteenth chapter of my work, without remarking how carefully they have concealed my experiments on the electrophorus. They have once indeed acknowledged, I have produced a variety of new experiments; but their candour never fails to give place to ſome ſecret or illiberal motive, whenever my experiments bear hard upon their favourite ſyſtem.

My eighth experiment, p. 154, proves, according to the Franklinian doctrine of luminous appearances, that the metal cover, when taken from an excited electrophorus, gives inſtead of takes a ſpark. I will venture to ſay, that this ſingle experiment effectually overthrows the Frankſinian doctrine of electric atmoſpheres, as well as the ſuppoſed laws of the directions of the ſtar and the bruſh. If this experiment had been as much in favour of their darling hypotheſis, as it apparently makes againſt it, it would, without doubt, have been ſelected for the inſpection of the public with an officious hand. Among the "great "number of original experiments," it is a little ſingular, that the friends to truth, and the well-wiſhers to the progreſs of real ſcience, ſhould ſearch in the fourth ſection of a miſcellaneous chapter, for two of the moſt intricate experiments they could pick out, when they might have ſelected, in the preceding ſection, an experiment which hardly requires any apparatus to perplex them. As theſe gentlemen have thought proper to with-hold every experiment, except my ſixth, which makes againſt the popular theory; and to place the three they have ſelected in the moſt unfavourable light; I could expect no other, than, after having reprobated my practice, they would condemn my apparatus. They are pleaſed to call it "a moſt complex apparatus," conſiſting of a variety of members, "certainly better adapted to "confound the author than to convert him."

I believe, with the Reviewers, there is hardly any part of my apparatus which can be applied to make a convert of me to the Franklinian doctrine; the longer I uſe it, the more I am convinced of the erroneouſneſs of the popular ſyſtem.

I have reaſon to think, that ſome of my apparatus has been adopted by electricians, and has anſwered; and if it ſhould happen to be about twenty times engraved, and as many times deſcribed by different authors, as great part of the electrical apparatus has been, it may then perhaps be as well underſtood as the machinery now in uſe. It does not always happen, that the inventor of every inſtrument in being hits on the moſt ſimple method of making it—time and ingenuity have frequently made improvements; but with liberal-minded men there is ſome merit allowed to the firſt inventor.

VIII.

They have been pleaſed to conclude their remarks, with ſaying, he "would undoubtedly have been more ſucceſsful in "this branch of philoſophy, had he not have proſecuted many "of his inquiries under the influence of prejudice."

A prejudiced perſon is ſeldom diffident of his cauſe: he expreſſes the reſults of his inquiries with confidence; he arrogantly propoſes his opinions for expreſs and poſitive laws; and he but ſeldom liſtens to the advice, or depends upon the judgment of another, even when he is undoubtedly wrong.

If I have, in the opinions of my adverſaries, expreſſed the reſults of my experiments without arrogance, I flatter myſelf I am not ſo much prejudiced as they pretend; and I think I may venture to ſay, without being thought preſumptuous, that this is an unwarrantable aſſertion; a declaration which one perſon ought not publicly to make of another, without the cleareſt conviction, from perſonal knowledge, of what has paſſed under his own eye.

As I have now pleaded to, and I hope weakened the principal objections of my adverſaries, I ſhall proceed to produce my experiments, as a farther proof of the permeability of glaſs to the electric fluid. For what I have ſaid, I expect no quarters from the candour of my antagoniſts. I make no doubt but they will dip their pen in their bittereſt gall, and dwell upon every ſlip of the pen with a ſecret delight; they may probably add the printer's ſins to mine: but when they have tired themſelves with finding fault, I can aſſure them they are no nearer the end of their labours than when they firſt began their animadverſions on my work; for it is reaſoning only, and not railing, which, will make a convert of me.

If, contrary to my expectation, I ſhould find the Monthly Reviewers, in their criticiſms on theſe ſheets, inclined to reject the abſurdities of the Peripatetics from their philoſophic creed, and to aim with a candid and laudable zeal to controvert reaſon with reaſon, and experiment with experiment, to eſtabliſh truth on an immoveable baſe, I ſhall then be convinced they mean to promote the cauſe of real ſcience, independent of party; and I ſhall hope, that in an impartial enquiry, error will be driven from its ſtrong and dark abode.

If in replying to the objections in the foregoing ſections, the candid reader ſhould think I have expreſſed myſelf too confidently on a difficult and controverted ſubject; I can aſſure him, I have not a wiſh to warp his judgment, nor a deſire he ſhould take any thing I have offered upon truſt. Let him, if poſſible, forget that either Dr. Franklin, or any other perſon, has ever wrote any thing upon this ſubject; and let him fairly and candidly try my experiments; and if he does not meet with ſucceſs, and draw the ſame, or ſimilar concluſions to mine, I ſhall then begin to think I have been deceived, by truſting too much to the child of my own brain.

FARTHER PROOFS, &c.

CHAPTER I.

Containing a ſet of experiments, with remarks, to ſhew the electric effluvia do paſs through glaſs: with a deſcription of the apparatus uſed in performing the Experiments.

IN plate I. ſee fig. 1. the repreſentation of a glaſs globe, about three inches diameter, blown thin, with a tube between two and three feet long, in which is put another tube, with a metal rod A, which projects beyond the end of the tube about two inches, and will admit a ſmall braſs knob on the end of it. The other extremity of the wire touches the internal ſurface of the glaſs globe at B; and as the wire is not confined, it may be ſlipped backward or forward at pleaſure.

Fig. 2. pl. I. ſhews a ſpiral tube, fixed in a ſmall foot, which is covered over the bottom with tin-foil, and in contact with that which goes round the tube. The ſlip of tin-foil, winding round the tube, has ſeveral diviſions, of about one twenty-fourth of an inch, to ſhew the electric fluid paſſing down the tube in diſcharging the jars.

The braſs cap C has a braſs wire, about four inches long, ſcrewed into it, with a metal knob D at the end of it.

The two jars, M and N, are fitted up the ſame as the Leyden-phial for electrical experiments, and, when uſed, are to be charged, one ſtanding at the conductor at the cylinder; the other at the conductor at the cuſhion, with a wire between them, reaching from coating to coating, as repreſented in the figure.

EXPERIMENT I.

To ſhew the electric effluvia paſſing through the ſubſtance of the glaſs in a ſtream.

Place the coated jar M at the conductor [*] G, at the cylinder; and the other jar N at the conductor H, at the cuſhion; and let the chain or wire O reach from coating to coating of the jars; ſee plate I. and fig. 1. of this work. Charge the jars; then take the glaſs globe, draw the end of the wire B about one fourth of an inch from the inner ſurface of it, and holding it by the tube with your right-hand parallel to the horizon, with the back of your left-hand near the external ſurface of the globe, and oppoſite to the end of the wire B; with the globe in this poſition, ſuddenly touch the knob of the jar M, with the end of the rod A, and you may ſee the electric effluvia ſtreaming from the point of the wire B, and paſſing through the ſubſtance of the glaſs in a copious ſtream; and you may likewiſe feel it on the back of your hand. If the point of the wire A be preſented to the knob of the jar N, the electric fluid may alſo be conducted through the globe to the back of your hand.

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[*] See plate I. in my Experiments and Obſervations, &c.

REMARKS.

As we are taught by the Franklinian doctrine, that we have but one way of leſſening or increaſing the quantity of electric effluvia in glaſs, and that is by coating the ſurfaces of the jar or the pane; it may be neceſſary to enquire, how this current of the electric fluid can be ſeen and felt, in the above experiment, in paſſing from the point of the wire to the back of the hand? If we can neither leſſen or increaſe the quantity of the electric fluid in uncoated glaſs; and the effluvia inherent in the pores of the glaſs ſtrongly repel and reſiſt any additional quantity; then this repulſive power ought to ſtop the electric particles, either at the point of the wire B, or before they touch the ſurface of the globe. If they impinge, they are abſorbed; and if abſorbed, it muſt be in the pores of the glaſs. By the popular theory, there can be no current of the electric effluvia from the point of the wire; yet, by this experiment, I produce one, if I can either ſee, or feel, or depend upon the evidence of my ſenſes.

The ſimple truth appears to be this:—the electric effluvia condenſed in the charged jar, by their repulſive power throw a part of the charge into the wire; and it is this repulſive property which gives a ſufficient velocity to the electric particles, at their firſt running, to overcome the attraction between them and the glaſs; and they paſs through the glaſs to the back of the hand, it being the neareſt conducting ſubſtance to the ſurface of the globe.

If it ſhould be aſked, why the whole charge does not run off in this inſtance? the anſwer is ready: It is owing to two cauſes:

1. The repulſive force of the electric effluvia, condenſed in the jar, appears to me to decreaſe in proportion as the charge of the jar is diminiſhed; for corpuſcular bodies attract and repel only at ſmall diſtances; and as the diſtances of the particles increaſe, the repulſive power will decreaſe.

2. As there is a ſtrong attraction between the electric fluid and glaſs; whenever the force of this attraction becomes greater than the power of the velocity of the electric fluid iſſuing from the charged jar, the current will be ſtopped by the attracting power of the glaſs, and the globe will charge, though there be no coating on either ſide of it.

EXPERIMENT II.

To ſhew the ſpark may be conveyed through glaſs, between the knobs of two jars differently charged.

Take the two jars M and N, and place one at the conductor G, the other at the conductor H, as in the firſt experiment, with a chain or rod between their outſide coatings. Charge the jars, and remove them as quick as poſſible from the conductors; for as ſoon as you leave off turning the cylinder, if the point in the conductor G be within ſix or ſeven inches of the cuſhion, I have frequently found great part of the charge would run off.

As ſoon as the charged jars are removed from the conductors, place them as at fig. 1. plate I. with a wire reaching from coating to coating; with the knobs of the jars rather farther apart than the length of the glaſs globe and tube A, B. Take the glaſs globe by the tube, and make uſe of it as a diſcharging rod, by bringing that part of the globe where the end of the wire B touches the internal ſurface of the globe, within ſtriking diſtance of the knob of the jar N; then, by ſuddenly touching the knob of the jar M, with the end of the wire A, a part of the charge of the jars will be diſcharged, with a ſpark and an exploſion; and, there being no conducting body near the external ſurface of the globe, the ſpark may be ſeen ſpreading on the globe, in different ramifications, before the electric fluid can be abſorbed by the atmoſphere.

EXPERIMENT III.

To ſhew the electric effluvia do not paſs from the jar M *to the jar* N, *to reſtore or ſupply any deficiency of the electric particles in the jar* N; *for they may be paſſed off by a conductor to the floor.*

Place the jars to the conductors, with the wire between them, as in the preceding experiment, and charge them. Warm the ſpiral tube, fig. 2. and place it with the knob D in contact with the knob of the jar N, and in ſuch a manner, that the braſs cap C is not farther from the knob of the jar M than the length of the globe and glaſs tube A, B, fig. 1. and 2. plate I.

Take the glaſs globe, inſtead of a diſcharging bow, and place the external ſurface of it oppoſite to the point of the wire B, in contact with the braſs cap C of the ſpiral tube fig. 2; then, by ſuddenly touching the knob of the jar M with the end of the wire A, and keeping your eye upon the ſpiral tube, the inſtant you hear the ſnap, you may ſee the electric effluvia ſtreaming down to the floor.

REMARKS.

Franklyn's
System of
the Leyden
Phial —

As theſe experiments are as new as they are ſingular, and will admit of but little quibbling reſpecting the direction of the electric fluid, whether it paſſes over the ſurface or through the ſubſtance of the glaſs; I ſhall endeavour to examine them candidly; for truth will bear the ſtricteſt ſcrutiny, though it cannot always prevail, when prejudice ſits as judge. If I underſtand the Franklinian ſyſtem of charging and diſcharging the Leyden-phial, the jar M will, in the two preceding experiments, be poſitively, and the jar N negatively charged, according to the popular theory. The electric fluid (according to the Frankliniſts) condenſed in the jar M, at the conductor G, by ſome occult property repels the electric fluid from the outſide ſurface of the jar M; and that paſſing along the rod O, is in its turn condenſed on the outſide of the jar N, at the conductor H; which alſo, by ſome antipathy, repels the electric fluid from the inſide ſurface of the jar N to the cuſhion. By this ſtrange and unknown property of the electric effluvia, acting through an impenetrable barrier, we have the outſide of the jar M negatively charged, and the outſide of the jar N poſitively charged. The way to reſtore the equilibrium of the two jars at once, is by forming a communication between the two jars, with ſome conducting ſubſtance; and the electric particles condenſed in the jar M, by their repulſive power forcing themſelves along the conducting bow, return to the inſide of the jar N, and, as they were repelled or driven out of the jar N by the electric fluid condenſed on its outſide, this ſuperabounding quantity is driven back again, and the jars are reſtored to their natural ſtate.

Specious as this hypotheſis may appear, I have reaſon to believe, and I think I ſhall prove, it ſtands upon a foundation ſo weak, that it muſt fall, when cloſely examined, and tried by experiments. I have already ſhewn, Exp. II. p. 32. that, according to the doctrine of the Frankliniſts, when a communication is formed between the knobs of the two jars, the ſpark paſſes from the knob of the jar M to the knob of the jar N. By this method of reaſoning it muſt be allowed me, that in this ſecond experiment, the ſpark entering the point of the wire A, will be conducted into the cavity of the globe, and, the glaſs being an impenetrable barrier, it cannot paſs any farther than the internal ſurface of the globe, and it muſt there be confined, if it cannot paſs through the glaſs. Are we to truſt to the evidence of our ſenſes, or to a theory which contradicts it? In this experiment under conſideration, I not only hear the exploſion, but I ſee the ſpark ſpreading on the ſurface of the globe, and, if I make my body a part of the conductor, I can feel the ſhock; yet I am to give up the privilege of reaſoning, and judging from what I ſee, hear, and feel, becauſe it claſhes with an hypotheſis propoſed in the infancy of this ſcience, and ſince embraced and ſupported by almoſt every electrician, becauſe it is eaſier treading in the ſteps of others, than tracing out a new path for ourſelves. In the third experiment, as well as in the ſecond, prejudice itſelf muſt allow the paſſing of the ſpark from the knob of the jar M to the end of the wire A; but, inſtead of going

Franklyn's System disputed—

to the jar N, to reſtore the ſuppoſed deficiency of the electric fluid, it is part of it led from the ſurface of the glaſs globe down the ſpiral tube to the floor. If there be no conducting body near the ſurface of the globe, then the electric fluid which iſſues from the jars is partly abſorbed by the atmoſphere.

As I intend to throw more light upon theſe experiments, in ſhewing the direction of the electric ſpark in diſcharging the Leyden-phial, I ſhall omit ſaying any thing farther upon the ſubject at preſent.

To obviate any controverſy with the real friends of ſcience, who care not who finds truth, provided it be found, and freely communicated, I ſhall next produce a very ſingular experiment, which may perhaps require more art, than prejudice can muſter, to ſuppreſs it.

EXPERIMENT IV.

To fire ſpirits of wine through the ſubſtance of glaſs.

Take a large metal ſpoon, and fill it with ſpirits of wine; heat the ſpirits, by holding the ſpoon over a candle, till they begin to evaporate plentifully, and then ſet them on fire; after they have burnt ſome time, blow out the light.

Charge the two jars, and, when charged, place them as in the ſecond experiment, with the rod O between their outſide coatings. Let a perſon hold the ſpoon with the ſpirits of wine cloſe to, or upon the knob of the jar N. Take the glaſs globe, (ſee fig. 1. plate I.) and bring that part of it oppoſite to the end of the point of the wire B, over the ſpirits, ſo near that it almoſt touches; then, by ſuddenly touching the knob of the jar

M with the other end of the wire A, the ſpirits of wine will, if the experiment be well made, take fire [*].

REMARKS.

If the electric ſpark does not in this inſtance paſs through the ſubſtance of the glaſs, I muſt call upon the advocates for the Franklinian ſyſtem to come forward with their whole tribe of ſympathies, and antipathies, and occult qualities; and ſhew us how one body acts upon another, through an impenetrable barrier: but I am apprehenſive, that all the abſurdities of the Peripatetics united, can hardly be found ſufficient to ſatisfy a friend to the Newtonian philoſophy, (who has been taught to ſeek a cauſe for every effect;) how the ſpirits of wine take fire. otherwise than through the Glass.

EXPERIMENT V.

To ſhew the electric ſpark may be conveyed through the ſubſtance of glaſs, in the circuit from inſide to outſide of the jar, and light ſpirits of wine in its paſſage.

Prepare a glaſs ſtand A, fig. 3. plate I, as high as the jar B, ſtanding on the ſtool C with glaſs legs, with its knob in contact with the knob of the prime conductor.

Round the glaſs tube A, fix a ſocket D, to ſlip up and down, and to be fixed at a proper height at pleaſure; on this ſocket fix another, parallel to the horizon, large enough to admit a glaſs tube E, through which paſſes a bended rod G, with a knob to it, and fixed as in the figure.

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[*] As this is an exceeding delicate and difficult experiment to perform, I hope no practitioner will condemn it too haſtily, if he ſhould not ſucceed; for I have repeatedly tried it with ſucceſs.

As I have not yet invented any inſtrument with which I can conſtantly ſucceed, I chuſe to give the experiment as I firſt made it.

Place the end of the rod H near the coating of the jar, and with a ſmall rod K, reaching from the curve G, while the jar is charging.

When the jar is charged, take away the wire K from the curve G, and ſlip the point H cloſe to the coating of the jar, and let a perſon take the ſpoon with the warm ſpirits of wine, and hold upon the knob of the rod G; then take the glaſs globe, with the rod A, B, fig. 1. and place the ſurface of the globe oppoſite to the point of the rod B, over the ſpirits of wine, ſo as the globe may almoſt touch the ſpirits of wine, but not the edge of the ſpoon. Let the end of the rod A be ſuddenly brought to the knob of the jar B, or to the conductor, which ſuits beſt; and the ſpark, if the experiment be well made, will ſet the ſpirits of wine on fire.

REMARKS.

As this experiment has two edges, it is a dangerous weapon for thoſe to handle who are not friends to truth. While the jar is charging, the electric effluvia will be ſeen on the end H of the rod, in form of a bruſh; yet I apprehend the moſt zealous advocate for the Franklinian doctrine will not be bold enough to grant me, that the electric fluid is in this inſtance ſetting in to the coating of the jar. As this will entirely deſtroy their doctrine of the negative ſtate of the outſide of the jar, I am ſure they will not readily expoſe themſelves to this difficulty; in which, if they are once entangled, they will not be able eaſily to diſentangle themſelves. As the electric particles are paſſing off by this rod, while the jar is charging, I muſt now aſk them, and that plainly, What becomes of their ſuppoſed law of indication? I here not only aſk the Frankliniſts in general, but the Monthly Reviewers in particular, Whether the ſeventh chapter of my firſt publication is not "adapted to overturn "Dr. Franklin's ſimple and luminous explanation of its ſingu"lar properties?" In the partial diſcharge, in this inſtance, we hear the exploſion, ſee the ſpark, and the ſpirits of wine on fire; and if the ſpark paſſes, as it is agreed it does, from the inſide to the outſide, it muſt paſs through the ſubſtance of the glaſs. As only a part of the charge is taken from the jar, this is a proof the globe is not perforated; for if it had been perforated, the whole charge would have been taken off.

It will be in vain to ſay, in this caſe, that the ſpark being led into the internal cavity of the globe, it repels the electric fluid from the external ſurface of the globe; for the globe is uncoated. Dr. Franklin ſays, in expreſs terms, "[a] We have "not any way of moving the electric fluid, but one, and that "is by covering a part of the two ſurfaces of thin glaſs with "non-electrics." Father Beccaria confirms what Dr. Franklin ſays, "A body, in order to be capable of receiving a charge, "muſt be inſulating; that is, of ſuch a nature as not to allow "the electric fire to move from one place of it to another, over "its ſurface."

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[a] Dr. Franklin's Experiments, &c. p. 7.

EXPERI-

EXPERIMENT VI.

Similar to the third Experiment, page 33;

To ſhew the electric ſpark, in diſcharging the Leyden-phial, does not paſs from the inſide of the jar to the outſide, to reſtore the ſuppoſed deficiency of the electric fluid; for it is either abſorbed by the atmoſphere, or paſſes off by a conductor to the floor.

Let the knob of the ſpiral tube D, fig. 2. plate I. be placed cloſe to the knob of the curved wire G, fig. 3. plate I. and let the ſtand and jar remain as in the laſt experiment. Turn the cylinder, and while the jar is charging, the electric fluid may be ſeen ſtreaming down the ſpiral tube. When the jar is charged, take the glaſs globe as before directed, and take a ſpark from the knob of the jar; and at the inſtant you hear the ſnap, if you keep your eye upon the ſpiral tube, you may ſee the fire in motion in every break of the tin-foil, and paſſing off from the glaſs globe, and the jar, to the floor.

REMARKS.

In the fourth and fifth experiments of the tenth chapter of my firſt publication (which the Reviewers "ſtudied more than once "from the beginning to the end," and I fear with no little diſpoſition to cenſure) I proved the electric fluid in the diſcharging did not return to the outſide of the jar, to reſtore any deficiency, but paſſed on to the table or floor of the room.

In the experiment now under conſideration, it is abſolutely impoſſible to give a rational account of the appearance of the electric fluid, in motion in the ſpiral tube, by Dr. Franklin's hypotheſis.

The ſpark which is communicated from the jar to the rod A, B, either paſſes through the ſubſtance of the glaſs, or is condenſed in the cavity of the globe. Say it is confined in the globe, and what will enſue, if we reaſon from the popular theory? The electric effluvia condenſed on the internal ſurface of the glaſs, will repel an equal quantity from the external ſurface; and the effluvia, thus repelled, ought to puſh inſtantly to the outſide of the jar B, fig. 3. plate I. to replace as much on the outſide of the jar as the inſide has delivered; but inſtead of this, we ſee them paſſing down the ſpiral tube.

If it ſhould be objected, that the electric particles in motion on the ſpiral tube, are running up it from the floor, to reſtore the equilibrum on the outſide of the jar B; this may paſs in theory, but it can never be admitted in practice, by thoſe who examine nature, and pay a particular attention to her fixed and regular laws.

As the motion of the electric fluid, from inſide to outſide of the jar, is conſidered as nearly inſtantaneous, the two currents, the one from the external ſurface of the glaſs globe, and the other from the floor, meeting, and action and re-action being equal in bodies moving with equal velocities, and containing equal quantities of matter, they muſt ſtop each other, and never reach the outſide of the jar.

But I will go farther, and admit that both currents ſet in to the outſide of the jar; then the outſide of the jar muſt receive more of the electric fluid than the inſide has parted with; or, the outſide of the globe cannot have loſt ſo much as the inſide has received: in either caſe the Franklinian doctrine contradicts

itſelf;

itſelf; therefore I ſhall not ſcruple to ſay, the electric fluid, in diſcharging the Leyden-phial, does not paſs on to the outſide, to reſtore the ſuppoſed equilibrium, but is abſorbed by the firſt conducting ſubſtance near the jar.

EXPERIMENT VII.

To ſhew the electric fluid paſſing away through a pane of uncoated glaſs;

Which will ſerve to prove my ſixth experiment, which the Monthly Reviewers were pleaſed to ſay, " they might content themſelves " by giving it a flat contradiction;" which is certainly an eaſy way of getting clear of a difficulty.

Take two jars, M and N, of the ſame ſize, plate I. fig. 4; coat the jar N, on the outſide only, with tin-foil cut in the form of rhomboides, with their points taken off, and fold them over the edge of the glaſs on the top. Cover the bottom of the jar N with tin-foil, and take care the bottom coating touches the rhomboids on the ſide of the glaſs. Have in readineſs four other jars or glaſſes, W, W, W, W, of the ſame height of the jar N, to place a pane of crown glaſs P upon (ſee the figure) of one, two, three, or four feet ſquare. Fix the jar N exactly in the center, and under the pane of glaſs. Over it fix the jar M, with its knob in contact with the knob of the prime conductor. When the apparatus is properly fixed, turn the cylinder of the machine, and as long as the jar is charging, the electric fluid may be ſeen, at every interval of the tin-foil, upon the jar N, running down to the table. Take the ſpiral tube T, and put it under the pane, holding it in your hand, with one end in contact with the tin-foil on the outſide of the jar N; and as long as the jar will receive any fire, the electric particles may be ſeen running along the tube to your hand.

As ſeveral perſons think the electric effluvia, in this experiment, cannot paſs through the glaſs, but muſt run over the edges of the pane, though they can give no reaſon why they ſhould paſs over the edges, I will, if I can, even ſatisfy their ſcruples; but as for thoſe who ſay they will not be convinced (as ſome ſuch I have met with) I do not think it worth while to trouble myſelf with them.

Inſtead of uſing the firſt pane of glaſs, P, in this experiment, take another, but let it be two feet or more ſquare, coat it round the edges with tin-foil on both ſides, half an inch wide, and place it upon the glaſſes, as before directed, with the jar M upon it. To every edge of the pane place a metallic conducting rod, with a braſs knob to each of them, and exactly the height of the pane from the table; when the apparatus is ready, turn the cylinder, and the electric particles may be ſeen running down the jar in the center, and under the pane, as before. It may alſo be conducted along the ſpiral tube, and the ſhock may be received by forming a communication between the knob of the jar M and the jar N, though the ſuppoſed impenetrable barrier be between them.

If the electric fluid in this experiment paſſed over the ſurface of the glaſs, it would, as ſoon as it reached the coating on the edges of the pane, be conducted to the floor by the metal rods in contact with the coating on the margin, and could not turn round to the under ſurface of the pane to the jar in the center. I ſhould think it trifling, to dwell upon ſuch remarks, if they had not been made to me by thoſe who had rather depend upon any weak ſuppoſition for a ſupport, than give up a doctrine which they have been taught to believe muſt be true.

Though I have contented myſelf with ſaying the electric effluvia may be paſſed through the ſubſtance of a ſingle pane of glaſs, I do not wiſh to have it underſtood, that this is all that can be done; for I have reaſon to believe I can do more. If I have, by the foregoing experiments, proved the electric fluid paſſes through glaſs, this is enough for my preſent purpoſe; and I ſhall next endeavour to ſhew the direction of the fluid in charging and diſcharging of jars.

CHAPTER

CHAPTER II.

Containing a new ſet of experiments, with remarks on them, to ſhew the direction of the electric fluid in charging and diſcharging coated jars.

A DESCRIPTION *of the* APPARATUS.

FROM the two conductors G and H, ſuſpend the two jars M and N, ſee fig. 1. plate II; and for a more particular account ſee chapter V. of my firſt publication.

On the top of the glaſs ſtand A, rather higher than the bottom of the jars from the table, fix on the oval metal plate C, by which has a ſocket underneath it, and in the center. This plate is between ſix or and ſeven inches long, and four or and five wide; but the exact dimenſions is of no conſequence in the uſe of it.

At each end of the plate C is a ferrel, large enough to admit the ends of the middling-ſize glaſs tubes D and E, through which paſs two ſmaller but longer glaſs tubes F and G, and theſe receive two metal rods H and I, which may be flipped backwards and forwards at pleaſure.

The ſmall tubes F and G, are fixed in the large ones D and E, by paſſing them through corks fixed in each end of the tubes D and E.

The double tubes are intended to prevent the electric fluid from paſſing in any other direction than along the rods H and I, in charging and diſcharging the jars.

In uſing this inſtrument, it is neceſſary to be provided with two panes of glaſs. One of them muſt be of the ſame width as the metal plate C, that it may be ſlipped between the two ferrels, and paſs under the ends of the glaſs tubes D and E. The pane which I have made uſe of I coated with pitch.

The other pane need not be more than ſix or ſeven inches ſquare.

In the following experiments, the balanced rods W and X are not uſed with the conductors, unleſs particular mention is made of them.

EXPERIMENT I.

To ſhew the direction of the electric fluid from the outſides of the jars, in diſcharging them.

Fix the inſtrument, as before deſcribed, fig. 1. plate II. between the two jars M and N, hanging from the conductors G and H, with the extreme ends of the rods H and I in contact with the outſide coatings of the two jars. The diſtance of the two points, H and I, can only be determined by practice; they muſt be regulated by the ſtrength of the charge.

Upon the glaſs pane P, fixed under the ends of the glaſs tubes D and E, ſtrew a little bran between the points of the rods, and let it touch each point.

Charge the jars as high as they will bear.

In diſcharging the jars, inſtead of placing the large conducting bow upon one conductor firſt, and then upon the other, to diſcharge the jars, lift up your hand over the conductors, and endeavour, with a ſudden motion, to touch both the conductors at the ſame inſtant with the two extremes of the conducting

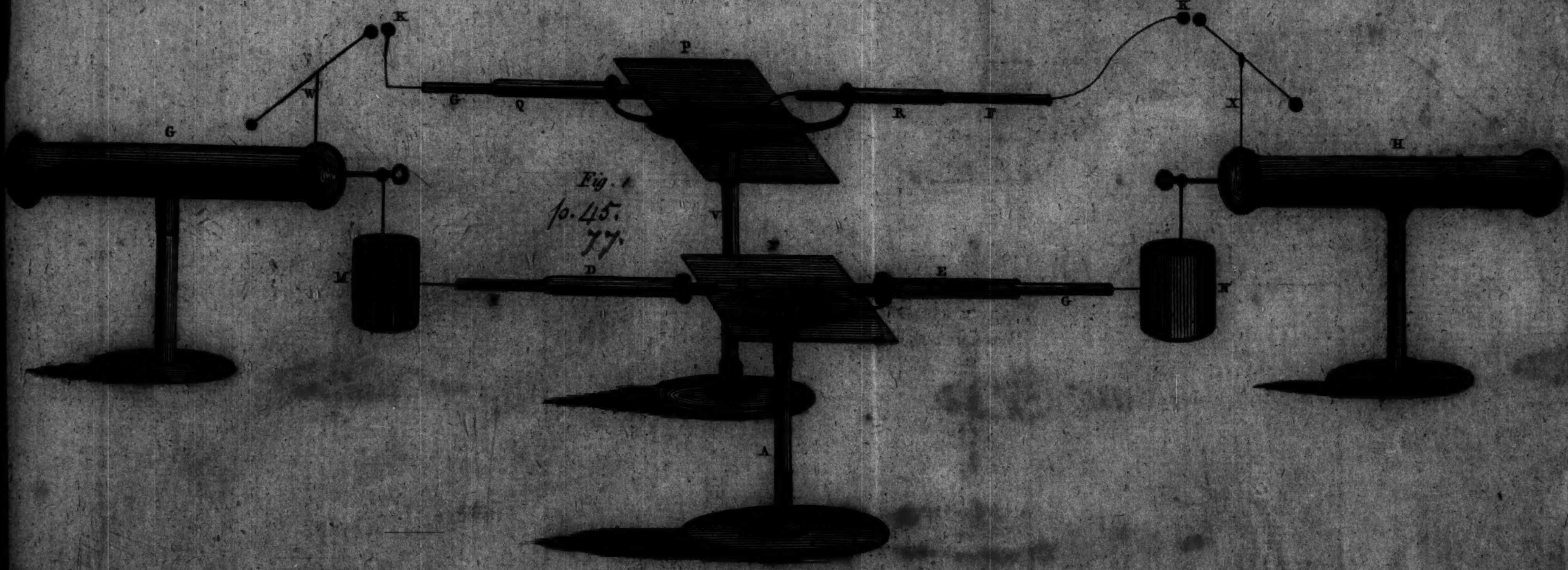

Fig. 1

p. 45.
77.

ducting bow; a diſcharge of both the jars will enſue, and a channel will be made through the bran upon the glaſs.

If the bran be ſtrewed upon the metal plate C, without the glaſs pane P, as in the firſt inſtance, and the jars diſcharged as before, the electric fluid iſſuing from the two points will repel the bran from under each point, and leave two circular vacuities in the bran upon the metal plate.

REMARKS.

In the firſt experiment, if the current of the electric fluid had been either from the point of the rod H to the point I, or from the point I to the point H, the bran would have been driven in the ſame direction with the ſtream of the electric fluid, and ſome of it would have been found at or near one of the ends of the glaſs tubes; but it is driven from each of them.

The bran is certainly carried with the ſpark; for light bodies do not move againſt the ſtream, unleſs they are impelled by a force which overcomes the ſtrength of the ſtream in or upon which they move.

As I am going to advance quite a new doctrine; and prejudice is ſo ſtrong againſt me; I mean to prove every, even the moſt ſelf-evident propoſition, as far as I can, by experiment.

EXPERIMENT II.

With a pith or a cork ball, to ſhew the direction of the electric fluid, from the outſide coatings, in diſcharging the jars.

Blow off the bran from the glaſs pane P, and let every part of the apparatus remain as before. Let the jars be charged, and place a cork or a pith ball at the end of the rod H. With the diſcharging bow touch firſt the conductor G, and before you can bring the other part of the diſcharger to the conductor H, to diſcharge the jars, the pith-ball will be driven from the point of the rod H towards the point I, and, if it happens to ſtrike the point I, it will be repelled by it, and driven towards the ſide of the glaſs. If the pith-ball be placed firſt at the end of the rod I, and the diſcharging bow be firſt placed upon the conductor H, then the reverſe will take place with the pith-ball.

REMARKS.

This experiment, with its reverſe, tends to prove, that the electric fluid, in the diſcharging the Leyden-phial, iſſues from the coating, and knob of the jar, at the ſame time.

For inſtance: When the pith-ball is placed at the end of the rod H, and you touch the conductor G, firſt, with one part of the diſcharging bow, the electric fluid in the jar M is firſt in motion, by running up the diſcharger, and at the ſame inſtant from the outſide or bottom of the jar along the rod, with a force ſufficient to drive away the pith-ball from the end of the rod H.

EXPERIMENT III.

To ſhew the direction of the electric ſpark in the diſcharging of jars, by cauſing it to mark its paſſage over the ſurface of a glaſs pane.

Change the two rods H and I for two others, long enough, when in contact with the coatings of the two jars, to meet in the middle upon the glaſs pane. Have in readineſs a pane of glaſs, ſix or ſeven inches ſquare, warm by the fire, and rub it on both ſides with a bit of tallow, about two inches wide, where you intend to mark the track of the ſpark. If the pane be cold, the tallow will adhere ſo faſt to the glaſs that the electric fluid cannot force the tallow before it; and if hot, the track will hardly be diſcovered: experience only can teach the operator what degree of warmth is neceſſary to perform this experiment with ſucceſs.

As ſoon as the apparatus is prepared, and the jars charged, put the pane with the tallowed edges between the wires. Let the point I, for inſtance, be under, and H upon the pane, and exactly oppoſite to each other. The diſtance of the points from the edge of the pane muſt be acquired by practice; it may be from an inch to two inches, accordingly as the jars act. Care muſt alſo be taken that both points of the rods touch each ſurface of the pane; this may eaſily be effected by bending one point a little up, the other rather down. When the apparatus is properly adjuſted, take the conducting bow, and ſuddenly touch both conductors at the ſame inſtant, which will form a communication between the inſides of the jars M and N, and diſcharge them. At the ſame inſtant that the knobs deliver each a ſpark, the wires will each of them give another ſpark, and in paſſing from the ends of the rods to the edge of the pane, will mark their tracks through the tallow, exactly oppoſite to each other.

I have ſometimes (but this is merely accidental, therefore I cannot give any directions to inſure ſucceſs) found, when there has been any little obſtruction on one ſide of the glaſs, where the ſpark has paſſed, it has divided the current, like a rivulet branching from a large ſtream, which has taken a little circuit, and united with the main body again before it reached the edge of the glaſs. I never yet found this diviſion on one ſide of the pane without finding it at the other, which to me is a deciſive proof of the electric ſparks iſſuing from the points of the rods at the ſame inſtant, and that their motions are regulated by each other by a ſtrong attraction. If the ſpark paſſes from one point to the other, over the ſurface of the glaſs, and turns the edge of the pane in its paſſage, what cauſe can there be aſſigned for its moving exactly oppoſite to the track it firſt made on one ſurface of the pane?

If the ſpark ſhould happen to be divided by any obſtruction on one ſide of the glaſs, why ſhould it divide on the other ſide, unleſs the obſtruction in ſuch inſtances always paſſes through the ſubſtance of the pane?

But the tracks made by the ſparks through the tallow are not always direct ones. Sometimes the ſparks ſhoot in oblique directions; but they always leave a kind of wavy tracks upon the glaſs, which are darkeſt in the middle. They who endeavour to account for theſe appearances, by ſuppoſing the point of the rod H to be negatively electrified, and the rod I poſitively electrified, and that the ſpark, in the diſcharging the jar, returns from the point I to the point H, to reſtore the equilibrium of the electric fluid on the outſide of the jar M, muſt advance a great deal of poſitive inconſiſtency, which can never be reconciled with the experiments I have, and ſhall offer under this head.

As the beſt method of inveſtigating difficult ſubjects in natural philoſophy, is by the ſlow and laborious method of making experiments, and drawing concluſions from them, I ſhall next try whether the ſparks from the knobs of the jars will not produce the very ſame appearances on the ſurface of the pane of glaſs.

EXPERIMENT IV.

To ſhew the direction of the electric ſparks, by cauſing them to mark their tracks over the ſurfaces of the glaſs pane, when they iſſue from the knobs of the jars in diſcharging them.

In preparing for this experiment, fix on the two balance rods, W and X, on the conductors G and H, and take care that each of the rods turn freely on their pivots.

Let the metal plate C, with all its apparatus, be taken off from the ſtand A, and fixed on the ſtand V, of the ſame height of the ſtands which ſupport the conductors G and H.

In the ſtead of the two ſtraight wires H and I, uſed in the third experiment, fix in the glaſs tubes F and G, the two bending rods H and I, with metal knobs to each of them, ſee fig. 1. plate II. Form a communication between the outſide coatings of the jars, with the curved wires M and N, ſee plate I. fig^{s}. 8, 8, &c. of my firſt work.

Place the glaſs ſtand V, with all its apparatus fixed as in the foregoing experiment, between the conductors, in ſuch a manner that the balance rods W and X, in turning on their centers, do not touch the knobs of the rods H and I; but mind they move within ſtriking diſtance, that they may diſcharge the jars. The exact diſtance muſt be acquired by practice.

As you turn the cylinder, the balance rods will move round on their centers; and when the jars are ſufficiently loaded with the electric fluid, by the meeting of the two knobs of the balanced rods at the knobs of the rods H and I, at the ſame inſtant they will each of them deliver a ſpark, which being conducted, by the bended rods to the points H and I, they will each of them mark their tracks through the tallow upon the pane, as before.

As in this experiment the tracks through the tallow are the ſame as in the laſt experiment, it will be unneceſſary to repeat what I have already ſaid, here; for where the effects are ſimilar, it is reaſonable to conclude the cauſes are the ſame.

Though I am certain that bigotted prejudice is at all times diſpoſed to combat the deductions of reaſon, I hope we ſhall not find it equally inclined to oppoſe itſelf to the direct teſtimony of the ſenſes.

EXPERIMENT V.

To ſhew that the direction of the electric fluid is from the outſide, as well as from the knob of the jar, in diſcharging the Leyden-phial.

Take the pane of glaſs, mentioned p. 42, foregoing, let it be dry, warm, and clean; place it upon the table, with the Leyden-phial upon the middle of it, with the knob of the jar in contact with the prime conductor. Turn the cylinder of the machine, and charge the jar. As the electric fluid is condenſing in the jar, you may ſee it ſhooting in corruſcations round the bottom of the jar to a conſiderable diſtance: ſometimes two inches or more.*

Clap the palm of your hand ſuddenly upon the knob of the charged jar, while ſtanding upon the pane, and you may ſee the ſame corruſcations ſhooting from the outſide coating, as there were while the jar was charging.

REMARKS.

It has not only been confidently [1] aſſerted, but generally believed, that a jar cannot be charged if inſulated, *viz.* ſtanding on an electric, unleſs a communication be formed between its coating and the floor.

That this ſhould have been received as a fundamental axiom by electricians, in the infancy of electricity, is not ſurprizing, as they hardly knew any thing more of the ſubject than how to collect the electric effluvia in a coated jar. As light has been gradually riſing upon this ſubject for upwards of forty years, it is aſtoniſhing that they ſhould blindly and implicitly follow each other ſo long in the beaten track, when the fallacy of this axiom might have been ſo eaſily detected by only placing a jar upon a pane of glaſs, and trying the reſult in endeavouring to charge it. They might alſo have ſeen, by placing their hand upon the knob of the jar, when charged, that the ſpark does not go from inſide to out, to reſtore the equilibrium; for the electric effluvia ſhoot from the outſide of the jar at the ſame time that they do from the knob: but this may be more clearly ſeen in the following experiment.

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[1] Dr. Franklin's Experiments and Obſervations, p. 24.——The phial will not ſuffer what is called a charging, unleſs as much fire can go out of it one way as is thrown in by another. A phial cannot be charged ſtanding on wax or glaſs, or hanging on the prime conductor, unleſs a communication be formed between its coating and the floor.

EXPERIMENT VI.

To ſhew the direction of the electric fluid in a more ſtriking manner, from the outſide coating, in diſcharging the Leyden-phial.

Place the pane of glaſs upon the five glaſs jars, as mentioned experiment VII. p. 42, and ſet the Leyden-phial upon the middle of the pane, with the knob in contact with the prime conductor. As ſoon as the jar is charged, with a diſcharging bow form the circuit between the inſide and outſide of the jar, and diſcharge it. In the act of diſcharging, by looking at the bottom of the jar upon the pane, the electric fluid may be ſeen pouring down, and running off by the diamond-coated jar, under the pane of glaſs, as in charging the jar. Though the electric fluid is ſeen ſtreaming down from the ſides of the jar, and paſſing through the pane to the floor, there is enough of it runs up the conducting bow to attract the ſpark from the knob of the jar.

REMARKS.

I have already ſhewn, p. 132, and experiment X. of my firſt work, that the electric effluvia, in diſcharging the Leyden-phial, paſs from the outſide coating through every conducting body in the circuit, and are at the extremity of it next the knob of the jar before a diſcharge can take place; and I here repeat it again, that upon this depends the exploſion and the ſhock; for if the current of the effluvia from the outſide coating be intirely ſtopped, no exploſion, with a ſpark and a ſhock, can take place. I ſhall dwell more fully upon this in the next chapter; but it may not be amiſs to ſhew by experiment here, that there is a ſtream of the electric fluid from the coating running up the diſcharging bow.

EXPERIMENT VII.

To prove it.

Charge the jar M, ſtanding upon the ſtool C with glaſs legs, ſee fig. 3. plate I. Take a diſcharging bow with a glaſs handle, ſee fig. 6. plate I. and put the lower part Q to the outſide coating of the jar upon the ſtool, and let the upper part R of the bow end with a blunted point. Let a perſon preſent any conducting ſubſtance to the knob of the jar, and let another preſent his hand near the point R of the diſcharging bow, and the electric fluid may be both ſeen and felt, as it is iſſuing from the point R of the conducting bow.

REMARKS.

Thoſe who offer to controvert ſuch proofs, and to deny the electric fluid to be iſſuing from the outſide of the jar at the ſame time it is iſſuing from the inſide; and contend for the ſparks going from the poſitive to the negative ſide of the glaſs, to reſtore the equilibrium; are no friends to the Newtonian ſyſtem of philoſophy; but are endeavouring, like ſome of the foreign philoſophers, to bury, in a heap of conjectures, and inconſiſtencies, which are contrary to the very evidence of our ſenſes, the principles, and laws of nature, as well as the rules of philoſophizing laid down in the Principia, by that great and able judge, Sir Iſaac Newton himſelf.

Till I tried the foregoing experiments, I never could explain, to my own ſatisfaction, ſeveral ſingular appearances which I have ſeen in trying different eſſays with the Leyden-jar.

I have already ſhewn, p. 48, that cork-balls are carried with the current of the electric fluid, in diſcharging coated jars; and that the direction of the ball depends upon which knob of the jar is firſt touched.

I ſhall under this head add two or three different experiments, which may not only ſerve to ſtrengthen the argument I have already advanced, but as a foundation to ground a rational explanation of what I intend ſhall follow.

EXPERIMENT VIII.

To ſhew, by the Leyden-phial attracting light bodies, the electric fluid iſſues from the outſide, at the ſame time it paſſes from the knob, in diſcharging it.

Hang a jar on each conductor, G and H, ſee fig. 6, 6, plate I. of my firſt work, and form a communication between their coating with the curved wires 8, 8; under the jar, on the conductor G, place a metal ſtand upon the table, and upon it a metal plate, which, with the aſſiſtance of a ſcrew, and a rod ſlipping in a ſocket, may be fixed up or let down at pleaſure, till it is fixed at a proper diſtance from the bottom of the jar. The diſtance will ſoon be found by a little practice. Upon the plate ſtrew a little bran, or put upon it a downy feather, or a pith-ball, or any other light body, which may be eaſily attracted by the charged jar. Charge the two jars as uſual. If you then touch the conductor G, the feather, pith-ball, or bran, or whatever the light body be upon the plate, will be attracted to

the

the bottom of the jar; and that by the electric effluvia iſſuing from it when you touch the conductor; and that upon the very ſame principle a feather, ſuſpended by a thread, is attracted by the conductor when charged with the electric fluid.

If, while the feather is hanging at the bottom of the jar G, you touch the conductor H, and take a ſpark from the knob of the jar hanging upon it, then the ſides or bottoms of both jars, throwing off a part of their electric fluid, and the feather being ſaturated with it, will fly off from the jar to the plate; and this attraction and repulſion (if it may be called ſo) of the feather, may be continued till the charges in the jars are ſo far exhauſted of their electric fluid, as not to have a ſufficient force to attract the light ſubſtances placed under them.

EXPERIMENT IX.

Suſpend a pith-ball with a fine ſilken thread, ten or twelve inches long, from an inſulating ſtand, and let the ball hang ſeven or eight inches from the upright, and within the atmoſphere of the jar, when charged at the conductor G. See fig. 5, plate I.

Charge the jars as in the foregoing experiment. When charged, then place the ſtand as above directed, with the pith-ball within the atmoſphere of the jar: then ſuddenly touch the conductor G; the pith-ball, by the ſhooting of the electric fluid from the outſide of the jar, will at firſt be driven a little from its perpendicular direction towards the ſtand; but as ſoon as the electrical particles have entered the pores of the ball, it will be drawn to the outſide of the jar.

This attraction of the pith-ball is cauſed by the electric fluid iſſuing from the outſide of the jar, and increaſing for the time the denſity as well as the dimenſions of its electric atmoſphere. I have ſometimes performed this experiment with the ball more than twelve inches from the jar.

CHAPTER III.

Containing a ſet of electric and magnetic experiments, to ſhew that all the phænomena in theſe two branches of ſcience are to be rationally explained by granting a polar virtue in each of their particles.

EXPERIMENT I. *In Magnetiſm.*

To ſhew the magnetic atmoſpheres, like the electric, change the property of each other, by the weakeſt receding from, or being repelled by, the ſtrongeſt atmoſphere.

TAKE a ſteel needle with a very fine point, and rub it from the eye to the point, five or ſix times, with the north pole of a magnetic bar; the eye will then be the north and the point the ſouth pole of the needle.

If the point of the needle be dipped in ſteel-filings, it will attract a conſiderable quantity round it. Take the magnetic bar in one hand, and the needle with the ſteel-filings in the other, and hold them parallel to the horizon, with the point of the needle near the ſouth pole of the magnet, and the ſteel-filings will fall from the point of the needle. As ſoon as the ſteel-filings drop off from the point of the needle, withdraw it from beyond the influence of the magnetic atmoſphere, and the point of the needle will be ſo far deprived of its attractive quality, that it will not again attract the ſteel-filings.

If,

If, inſtead of taking away the needle, as ſoon as the ſteel-filings drop off, you continue it three or four minutes about half an inch from the magnetic bar, the needle will be poſſeſſed of an attractive property, but its directive quality will be changed from ſouth to north; for if you dip the point of the needle into the ſteel-duſt again, it will attract a conſiderable quantity as at the firſt. In this caſe, if the point of the needle be now brought within the atmoſphere of the ſouth pole of the magnetic bar, the ſteel-filings upon the point of the needle, and the magnetic bar, will mutually attract, and be attracted; which proves the property of the magnetic effluvia in the point of the needle has been reverſed.

REMARKS.

In the foregoing experiment, and its variety, we find a magnetic atmoſphere, like an electric[*] one, introduces a contrary magnetic quality into a body immerſed in it; viz. the moſt powerful atmoſphere, by its ſuperior influence, reverſes a weak one, and changes it from north to ſouth, or from ſouth to north, as you apply the magnetic atmoſpheres of the ſteel bar to the point or eye of the needle.

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[*] Beccaria's Artificial Electricity. p. 186, ſect. 432.——I have examined into the firſt property of electric atmoſpheres, viz. that the electricity of a body endeavours, through the atmoſphere which it actuates, to introduce a contrary electricity into a body immerſed into it.

Two ſecondary properties neceſſarily proceed from the above:

1. When two bodies, impregnated with homologous electricities, meet, their electricities, by means of the atmoſphere which they actuate, endeavour reciprocally to deſtroy each other.

2. When two bodies, impregnated with contrary electricities, meet, theſe electricities, by means of their intervening atmoſpheres, reciprocally increaſe their intenſities.

" As we are not to recede from the analogy of nature, which " uſes to be ſimple, and always conſonant to itſelf, nor are we " to admit more cauſes of natural things than are both true " and ſufficient to explain their appearances." I ſhall therefore endeavour to apply the doctrine of electric atmoſpheres, to magnetic attraction and repulſion, and try if ſimilar effects can be rationally explained by it.

1. After the needle has been rubbed with the magnet, as mentioned in the preceding experiment, and has gained an attractive quality, I imagine it will be granted me it is poſitively and not negatively magnetical. This is a phraſe not known in this branch of natural philoſophy.

2. When two bodies, impregnated with homologous magnetic properties, meet, theſe magnetic bodies, by means of the atmoſpheres which they actuate, endeavour reciprocally to deſtroy each other. This I prove by ſteel-duſt falling off from the point of the needle, when brought within the atmoſphere of the magnet, and of the ſame quality of its own. For inſtance: If the point of a needle has a directive property to turn to the ſouth, it will be deprived of that property, by being brought near the ſouth pole of a ſtrong magnet; but if the needle be continued in the ſame poſition, it will acquire another attractive property, contrary to the firſt; and, if it had before a poſitive attractive quality, it will, by the laws of electric atmoſpheres, acquire a negative magnetic property.

3. To the ſame natural effect we muſt, as far as is poſſible, aſſign the ſame natural cauſe; and if electric atmoſpheres change, by ſome ſecret and occult quality, the electricity of bodies im-

merſed

merſed in them, the magnetic muſt do the ſame; for the point of a fine needle will, when held for a few minutes in a magnetic atmoſphere, acquire a contrary property to the atmoſphere in which it is placed.

I am confident that this doctrine of poſitive and negative magnetic properties will not be admitted by the philoſophers into their magnetic creed, to explain the foregoing magnetic appearances; and I believe it is as abſurd as it is inconſiſtent with the ſimple laws of nature, and by no means ſufficient to be received as a theory to explain the nature of electric and magnetic attraction and repulſion.

As there is ſuch a ſimilarity between magnetic and electric attraction, I do not deſpair of ſhewing that one ſimple and uniform principle will explain both; but, that I may not be too raſh in building without a foundation, I ſhall not advance any farther, till I have produced more proof.

EXPERIMENT II.

To ſhew the ſimilarity between the acting of electric and magnetic atmoſpheres.

Take a fine ſteel rod, about four inches long, of the ſize of a ſmall knitting-needle, and let it have two ſharp points, and a center to turn freely upon a pivot, which is fixed perpendicularly to the horizon, in a ſmall braſs foot upon the table. Rub the rod four or five times with a magnet, in the ſame direction, to give it a directive property, and then place it upon the end of the pivot. If you bring the north pole of the magnet near the north pole of the needle, or the ſouth pole of one near the ſouth pole of the other, the point of the needle will, in each caſe, recede from the end of the magnetic bar.

Place your finger upon the center of the needle, and ſtop it from turning, when you preſent the north pole of the magnet near to the north pole of the needle, or the ſouth pole near to the ſouth pole; and after holding the magnet three or four minutes oppoſite and near to the end of the needle, remove your finger from the center of the needle, and the needle will now ſtand and attract, and be attracted by, the magnet.

In this experiment the directive property of the needle is ſo far diſturbed or changed as to act differently, nay contrarily, to what it does in other caſes.

EXPERIMENT III.

To ſhew the ſimilarity between the electric and the magnetic atmoſpheres, in changing the property of an electric or magnetic body, brought within their ſphere of acting.

Rub a needle with a fine point, five or ſix times, from end to end, in one direction, upon the north pole of a magnetic bar. Place the needle upon the table, with the north pole oppoſite to the north pole of the magnet, and at ſuch a diſtance from it, that the magnet cannot draw the needle from its place.

After they have continued in this poſition an hour or two (the time muſt depend upon the ſtrength of the magnet) the attractive quality of the needle will be reverſed; the pole of the needle, which was at firſt north, will be changed to ſouth. I have frequently,

quently, by turning the needle, reverſed the poles of it ſeveral times.

REMARKS *on the two preceding Experiments.*

From the foregoing experiments, I think I may fairly draw the following concluſions:

1. That a ſteel needle, fixed with one of its points oppoſite to the end of a magnetic bar, and within its atmoſphere, though not in contact, will acquire an attractive quality.

2. That a magnetic needle, with ſteel-filings at the point of it, brought within the atmoſphere of a magnetic bar, will, upon the falling off of the filings, be deprived of its magnetic property.

3. If the needle be continued in the atmoſphere of the magnet, after the dropping off of the ſteel-filings, it will acquire a contrary magnetic property to that of the magnet.

4. In the firſt inſtance, the point of the needle is undoubtedly poſitively magnetical.

In the ſecond, when it is deprived of its magnetic property, it is in its natural ſtate.

In the third inſtance, where the attractive property of the needle is changed from north to ſouth, or from ſouth to north, accordingly as you apply the poles of the needle and the magnet near to each other, the point of the needle muſt be negatively magnetical, if we explain theſe changes by the law of electric atmoſpheres. But it may be anſwered, Steel, in its natural ſtate, has neither a directive, a repulſive, or an attractive property; and that theſe properties are acquired by rubbing, by ſtanding in a perpendicular direction, and in being placed in a magnetic atmoſphere; and that the ſteel thus rubbed, fixed, or held in a magnetic atmoſphere, acquires a polar virtue by the magnetic particles entering the ſubſtance of the needle, and that the directive property will be invariably contrary to the polar virtue of the effluvia iſſuing from the end of the magnetic bar.

As the magnetic particles appear to me to move in a regular order, with a north and a ſouth pole oppoſite to each other, and to arrange themſelves in this manner in the magnetic needle; that atmoſphere which is the ſtrongeſt muſt repel the weak one, when two homologous atmoſpheres are oppoſed to each other. But the ſtrongeſt atmoſphere will not only repel the weakeſt, it will even overcome the attraction between the effluvia and the ſteel needle, and force the effluvia back, and the particles of the ſtrongeſt atmoſphere will occupy their place, and conſequently give the point of the needle a contrary directive property, as the effluvia always arrange themſelves in ſteel with a north and ſouth pole oppoſite to each other; and wherever they are ſeparated, they mutually attract.

Upon this ſimple and univerſal principle depends, as far as I am capable of judging, corpuſcular attraction and repulſion. By admitting a polar virtue to the primary parts of matter, we may explain rationally, and I believe truly, all the phænomena of magnetic and electric attraction and repulſion; but by denying the firſt particles of matter this property of a polar virtue, we ſhall be driven by neceſſity to admit into our philoſophic creed many abſurdities, which were embraced in the dark ages of ignorance, when conjecture ſupplied the place of experiment, and the ſuppoſed ſympathies and antipathies of matter were conſidered as fixed and poſitive laws of nature. Sir Iſaac Newton, to whoſe happy penetration we owe the hint, aſks, "[1] Have not the ſmall particles of bodies certain powers, virtues, or forces, by which they act at a diſtance, not only upon the rays of light, for reflecting, refracting, and inflecting them, but alſo upon one another, for producing a great part of the phænomena of nature? It is well known that bodies act one upon another by attraction of gravitation, magnetiſm, and electricity: and theſe inſtances ſhew the tenor and courſe of nature, and make it not improbable but that there may be more attractive powers than theſe; for nature is very conſonant and conformable to herſelf."

He farther ſays, "It [2] ſeems to me, that theſe particles (*viz.* the primoriginal particles) have not only a VIS INERTIÆ, accompanied with ſuch paſſive laws of motion as naturally reſult from that force, but alſo, that they are moved by certain active principles, ſuch as is that of gravity, and that which cauſes fermentation and the coheſion of bodies. Theſe principles I conſider not as occult qualities, ſuppoſed to reſult from the ſpecific form of things, but as general laws of nature, by which the things themſelves are formed: their truth appearing to us by phænomena, though their cauſes be not yet diſcovered. For theſe are manifeſt qualities, and their cauſes only are occult. And the Ariſtotelians gave the name of occult qualities not to manifeſt qualities, but to ſuch qualities only as they ſuppoſed to be hid in bodies, and to be the unknown cauſes of manifeſt effects: ſuch as would be the cauſes of gravity, and of magnetic and electric attractions, and of fermentations, if we ſhould ſuppoſe that theſe forces or actions aroſe from qualities unknown to us,

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[1] Sir Iſaac Newton's Optics, p. 350 and 351.

[2] Ibid, p. 376, 377.

“ and uncapable of being diſcovered and made manifeſt. Such occult qualities put a ſtop to the improvement of natural philoſophy, and therefore of late years have been rejected. To tell us that every ſpecies of things is endowed with an occult ſpecific quality, by which it acts, and produces manifeſt effects, is to tell us nothing; but to derive two or three general principles of motion from phænomena, and afterwards to tell us how the properties and actions of all corporeal things follow from theſe manifeſt principles, would be a very great ſtep in philoſophy, though the cauſes of thoſe principles were not yet diſcovered; and therefore I ſcruple not to propoſe the principles of motion above-mentioned, they being of very general extent, and leave their cauſes to be found out.”

As electric and magnetic attractions are manifeſt qualities, and muſt be produced by ſome cauſe, I know of no cauſe ſo likely to produce theſe effects, or by which we may rationally explain the phænomena of electric attraction and repulſion, as by granting a certain active principle, or a polar virtue to corpuſcular bodies.

All corpuscular
bodies endowed with
polar Virtues —

In order to apply this doctrine to uſe, I ſhall take one or two of the moſt common experiments in electricity.

EXPERIMENT III.

To ſhew the nature of electric attraction and repulſion.

Take a pith-ball, ſuſpended with a fine ſilken thread, and holding it between your finger and thumb, bring it within the influence of the atmoſphere of the prime conductor, when charged with the electric effluvia, and it will be attracted by the conductor. As ſoon as the ball is ſaturated with the electric fluid, it will be repelled by the conductor.

REMARKS.

In this ſimple and common experiment, known to every electrician, the prime conductor emitting a great quantity of the electric effluvia, and their attractive quality being ſtronger than the gravity of the pith-ball, they overcome it, and draw it out of its perpendicular direction. The effluvia being cloſer, and more copious at little diſtances from the emitting body, light ſubſtances, ſuch as the pith-ball, when within the electric atmoſphere, will be drawn towards the denſer effluvia with an accelerated motion, till it adheres to the emitting body itſelf.

The pith-ball, in this caſe, being ſuſpended by a ſilken thread, which being an electric, by its attracting and abſorbing the electric fluid, and ſtopping it from paſſing off as faſt as it is condenſed on the ball, the ball becomes ſaturated with the electric effluvia; and when it can contain no more, it is repelled by the conductor.

Some may ſay, that if the ball, in this inſtance, is held in what they call a poſitive atmoſphere, it will be deprived of its natural quantity of the electric fluid; but I do not ſcruple to ſay, that it as much acquires an additional quantity of the electric fluid, as a needle, rubbed with a magnet, gains a portion of the magnetic effluvia *.

Though the ball, when ſaturated with the electric fluid, mutually repels and is repelled by the conductor; if you force it

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* See Experiments from I. to X. on the electrophorus, p. 148, &c. of my firſt publication.

again into the atmoſphere of the conductor, and let a perſon continue turning the cylinder, the atmoſphere of the conductor, if vigorous, will ſoon act like the atmoſphere of the magnet on the point of the needle (ſee Experiments I. and II. p. 59 and 62, foregoing) and by repelling the particles of the electric fluid, which the ball acquired in its firſt contact with the conductor, farther up the ſilk will enter the ſubſtance of the ball again, and attract it, and it will again be repelled.

If the experiment be repeated again and again, by bringing the ball into the moſt denſe part of the atmoſphere of the conductor, it will, after two or three minutes, be attracted; and this attraction and repulſion may be continued as long as you pleaſe.

Relinquiſhing for a minute the ſuppoſed ſimilarity between magnetic and electric attraction and repulſion, I will endeavour to explain the attraction and repulſion of the ball by the Franklinian doctrine, and ſee what a curious ſucceſſion of changes from poſitive to negative, and from negative to poſitive, it will produce in the ball.

1. The pith-ball being brought into the atmoſphere of the conductor, we are told it is deprived of its natural quantity of the electric fluid, and becomes negatively electrified[5].

By the firſt general propoſition in the Franklinian ſyſtem, the pith-ball and the conductor ought mutually to attract and be attracted; but inſtead of this they mutually repel.

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[5] Dr. Franklin's Experiments, p. 155.——An electric atmoſphere not only repels another electric atmoſphere, but will alſo repel the electric matter contained in the ſubſtance of a body approaching it, and, without joining or mixing with it, force it to other parts of the body that contained it.

See Beccaria, p. 180.——The electricity of a body A actuates the ambient air in ſuch a manner, that by means of the ſame, it tends to introduce into the neighbouring body B, immerſed in it, an electricity contrary to its own.

2. To make the experiment agree with the propoſition, it will be neceſſary to grant, that the ball receives as much of the electric fluid from the conductor as it can contain, and is poſitively and ſimilarly electrified; then, like the two north or two ſouth poles of a magnet, they will repel each other. But this is contrary to the popular theory; therefore either the propoſition, or the doctrine of electric atmoſpheres, muſt fall to the ground.

3. From the preceding remark, p. 69, it is plain by the Franklinian theory, that if the ball in its natural ſtate be held in what is called a poſitive atmoſphere, it will then be negatively electrified. If in its negative ſtate it be brought again into the ſame poſitive atmoſphere, it muſt then be poſitively electrified, and ſo on alternately; yet the ball will, in every change from poſitive to negative, be attracted by the conductor, if the atmoſphere be powerful enough to overcome the reſiſtance of the electric particles in the pith-ball.

EXPERIMENT IV.

To ſhew the ſimilarity between the magnetic and electric attraction.

Take two circular pieces of paſteboard, A and B, ſee fig. 2. plate II. of about three inches diameter, cover them over with ſilk, and take care the oppoſite ſurfaces be ſmooth and even. Fix the back of each piece of paſteboard to the curved wire rods C and A, and after ſlipping out the wire rods H and I, from the glaſs tubes F and G, upon the glaſs ſtand A, fig. 1.

PLATE II.

Fig. 2

p.70.

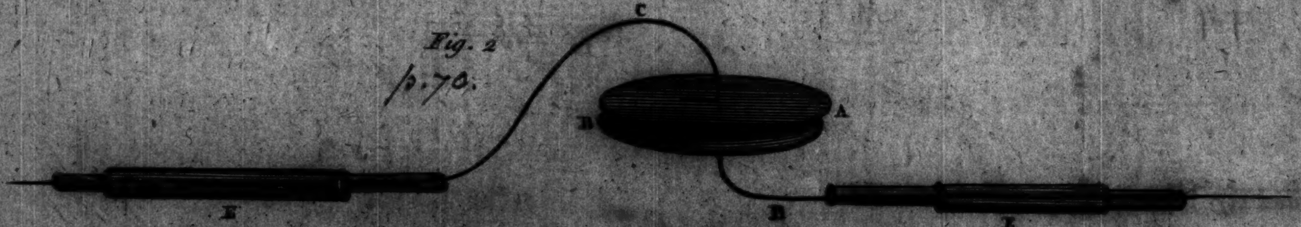

Fig. 3

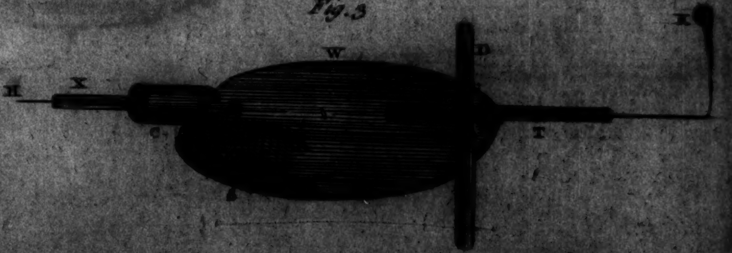

plate II. ſlip in the two curved rods in their ſtead, as in fig. 2; and when fixed upon the ſtand A, fig. 1. plate II. let the ends of the rods touch the outſide coatings of the jars M and N, and take care the two covered plates A and B be exactly over each other. The diſtance from the two oppoſite covered ſurfaces muſt be regulated by the ſtrength of the current of the electric fluid. Upon the lower plate B, ſtrew ſome bits of paper, cut very ſmall, the leſs the better. When the apparatus is properly prepared, let the cylinder of the machine be gently turned, and the ſmall bits of paper will form into ſtrings, like ſteel-filings, between the north and ſouth poles of magnetic bars.

If the experiment ſhould not ſucceed well, by reaſon of one or two rows of the paper forming a communication between the upper and lower ſurfaces of the plates A and B, while the cylinder is turning; as ſoon as there is a little fire condenſed in each jar, ſtop the cylinder, and by ſuddenly touching the conductor G, the paper will be all in motion, and form a great many ſtrings, by joining one to another between the two ſurfaces.

REMARKS.

If we conſider that this experiment is ſimilar to one of dipping the north and ſouth poles of two magnetic ſteel-bars into ſteel-filings, and bringing them near each other to ſee the filings unite, it will be as unphiloſophical as it will be erroneous to aſcribe two different cauſes for almoſt the ſame appearance. It appears to me, that whatever is the cauſe of the ſteel-filings forming themſelves into rows between the north and ſouth poles of the magnetic bars, the very ſame principle cauſes the paper to form into rows between the two ſilken ſurfaces A and B, ſee fig. 2, as before.

If the magnetic experiments are explained by admitting a polar virtue in the magnetic effluvia, we muſt grant the ſame to the electric particles, or call in the aſſiſtance of a great deal of abſurdity to help us out of our difficulties, in many phænomena which occur in our electric purſuits.

I will venture to ſay, that the doctrine of a polar virtue in the primoriginal particles of matter, is far more philoſophical and natural than the Franklinian ſyſtem; it is equally as ſimple, and may be rationally applied in explaining all the different electric phænomena: but to purſue this track would carry me far beyond my preſent deſign.

EXPERIMENT V.

To ſhew that the diſcharging of the Leyden-phial with a ſpark and a ſhock, is owing to the polar and attractive quality of the electric particles, and not to any deficiency of the fluid in the outſide of the jar.

Take a jar, and charge it as uſual at the conductor G; and as it is ſtanding upon the table, put the lower knob of a diſcharging-bow to the outſide coating of the phial, and keep the upper knob at ſome diſtance from the knob of the jar. Let another perſon take a metal rod, with a knob of the ſame ſize as the knob of the diſcharging-bow, and let one be at one ſide, and the other at the oppoſite ſide of the knob of the charged jar. If theſe knobs are both brought towards the knob of the jar, and at equal diſtances from it, as ſoon as they are within ſtriking and equal diſtances, the ſpark will fly from the knob of the jar to the diſcharging-bow, and not to the knob of the metal rod leading to the floor.

REMARKS.

In this ſimple experiment, there muſt be ſome reaſon why the electric ſpark flies to one of the knobs rather than the other. The Frankliniſts will tell you, the inſide of the jar cannot give a ſpark, without the outſide can receive it, in diſcharging a jar; but this to me is by no means a ſatisfactory anſwer. There muſt be ſome cauſe or firſt mover to unite the force of the ſeveral parts of an electric jar, and to direct it to one point rather than to another. We know both condenſed and rarefied air, confined in a narrow ſpace, will find a paſſage through the weakeſt part of any thing which contains it; but in the caſe of the Leyden-phial, though it be open on the top, and a metal rod with a knob to it held near the knob of the jar, the charge will not run off, unleſs there be a communication between the inſide and outſide coatings.

The ſuppoſed negative ſtate of the outſide of the jar, can no more firſt put the charge in motion, than an empty bottle can receive the contents of a full one, with a ſyphon in it, before the air is exhauſted from the upper part of the ſyphon. As this appears to me to be a real matter of fact, I ſhall proceed to enquire, whether the Franklinian doctrine of electric atmoſpheres be ſufficient to remove the difficulty, and to give a ſatisfactory ſolution, to a perſon who is ſearching after a cauſe for every effect, and would not willingly take any thing for granted, if there be a poſſibility of proving it.

It appears to be a ſettled point with the advocates for the Franklinian ſyſtem, “ [6] That the electric fire, which is accumulated in a body, endeavours to drive the natural fire from another body that is not electrified,” and to render it deficient of the electric fluid.

By this doctrine, and by what Dr. Franklin [7] ſays in anſwer to his correſpondent's queſtion concerning the Leyden-phial, I cannot conceive how a diſcharge can take place. But I will endeavour to explain myſelf more fully, in applying this method of reaſoning, ſo far as I underſtand it, to uſe.

Let a jar be charged in the common manner: and take a ſmall diſcharging-bow with a glaſs handle; put the lower knob of the bow to the outſide coating of the jar, and bring the upper knob of the bow in the atmoſphere of the knob of the jar, but not within ſtriking diſtance. In this caſe the upper knob of the bow becomes negatively electrified, according to the Franklinian doctrine; *viz.* the natural quantity of the electric fluid, inherent in the upper part of the bow, is ſuppoſed to be repelled, or driven down the bow. But where is it driven to? It cannot go to the outſide of the jar; for, according to Dr. Franklin's own account, while the repulſive power remains in the jar, by the charging it, the bow cannot, any more than the hand, give any fire to reſtore the equilibrium.

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[6] Beccaria's Artificial Electricity, p. 185.

[7] Dr. Franklin's Experiments and Obſervations, p. 461.——The hand that holds the bottle, receives and conducts away the electric fluid that is driven out of the outſide by the repulſive power of that which is forced into the inſide of the bottle. As long as that power remains in the ſame ſituation, it muſt prevent the return of what it had expelled, though the hand would readily ſupply the quantity, if it could be received.

As there is ſuch a ſtrong repulſive force acting againſt the lower part of the conducting bow, to oppoſe the current of the electric fluid from ſetting in to the outſide of the jar; and is equal to the force within the jar; I cannot conceive, by this doctrine, how the ſpark can be collected, or how a diſcharge can take place.

But I have already ſhewn, chap. X. p. 132 of my firſt publication on this ſubject, that the electric fluid paſſes from a charged jar through every conducting body in the circuit, and reaches even to the very extremity of the circuit next to the knob of the jar, before a diſcharge with a ſpark and a ſhock can take place. Whenever the electric fluid is paſſed through glaſs, as in charging the Leyden-phial, the effluvia contained in the pores of the glaſs will, like a magnetic bar, always cauſe the two ſurfaces of the jar to have different polar virtues. If there be any better conducting ſubſtance than the table, in contact with the outſide of the jar, to lead off the electric fluid, it will continue its property through the uninterrupted part of the circuit for any diſtance; and when the extreme part of the circuit is brought near the knob of the jar, there will be a very ſtrong attraction between the particles of the electric fluid in the jar, and in the circuit leading from the outſide of it; the attraction between the particles will be ſtronger than between the particles and the glaſs; it is this attraction which firſt puts the condenſed effluvia in the jar in motion, it cauſes them inſtantly to unite, and, when the knob of the diſcharging-bow is within ſtriking diſtance of the knob of the jar, a diſcharge will inſtantly enſue.

Electric Matter
has two polar
Virtues ——

The Electric matter iſsues from both sides of the Jar at the same time in the discharge

This ſhews us why the ſpark, in the diſcharging of the jar, tends to the upper knob of the diſcharging-bow, rather than to the other knob, out of the circuit. But the ſpark does not go to the outſide of the jar, to reſtore any equilibrium or deficiency of the electric fluid, as has been generally ſuppoſed; for the electric effluvia iſſue from the bottom of the jar at the ſame inſtant they do from the top, or, in other words, from the outſide at the ſame inſtant they do from the inſide, as I ſhall prove in the following experiment.

EXPERIMENT VI.

To ſhew the electric effluvia ſtreaming from the outſide of the jar in the diſcharging of it.

Take the apparatus deſcribed, Exp. VII. p. 42, and fix it up (ſee plate I. fig. 4.) and charge the jar M, ſtanding upon the pane of glaſs P, with the jar N and the ſpiral tube T under the pane. As ſoon as the jar M is charged, place your hand ſuddenly upon the knob of the jar; it will deliver a ſpark; and at the ſame inſtant you may ſee the electric effluvia ſtreaming down from the bottom of the jar, the ſame as it does in charging, and it will paſs through the pane of glaſs P, down the ſide of the jar N, and along the tube T, to the hand of an indifferent perſon.

REMARKS.

As the electric effluvia appear to me to iſſue from the bottom of the jar, through the glaſs, and along the ſpiral tube, and likewiſe up a conducting-bow, as I have proved Expt.

VI. and VII. p. 54 and 55 foregoing; I hope the Frankliniſts will in future fairly and candidly examine what I have offered, before they reprobate a work founded upon experiments, and the reſults from which are fairly and candidly deduced from them.

I have already hinted, in the fifteenth chapter of my firſt work, that natural as well as artificial exploſions may be owing to one and the ſame cauſe. When two clouds, loaded with electric fluid, get within the ſphere of each other's attraction, they approach each other with an accelerated motion, and the velocity of the electric fluid, at the inſtant of their exploſion, will be ſufficient to light inflammable bodies near them.

There are ſeveral methods of firing inflammable ſubſtances in diſcharging of jars; and if art can do this, what cannot nature produce?

As I have, as far as I know, ſomething new on this ſubject, I ſhall here offer it in proof of what I have ſaid.

EXPERIMENT VII.

To light ſpirits of wine by the paſſing of ſparks from knob to knob, and from outſide to outſide, of the jars M *and* N.

Take the apparatus (ſee fig. 1. plate II.) as deſcribed p. 46 and 51 foregoing. Remove the glaſs panes P, P, and in their place put a ſmall ſaucer between the points of the wires H, I, and H, I, and in which let there be put ſome warm ſpirits of wine. Bend the points H, H, of the wires, that they may dip in the ſpirits, and the points I, I, that they may almoſt touch the ſurface of the ſpirits in each ſaucer. With a ſlip of paper light the ſpirits of wine, and after they have burnt about a minute, blow out the flame. Charge the jars as uſual; the balance rods will turn upon their pivots W and X, and as ſoon as their knobs both meet at the ſame time, at the knobs of the bended wires K, K, they will each of them deliver a ſpark, and ſet fire to the ſpirits in the ſaucer upon the ſtand W.

A ſpark will likewiſe iſſue from the outſide of each jar, and light the ſpirits in the ſaucer upon the plate C, at the ſame inſtant.

REMARKS.

As the ſpirits of wine are lighted between the outſide coatings of the jars M and N, as well as between their knobs, it is evident the ſparks, in the diſcharging of the jars, paſs with as great a velocity from the outſides of the jars as from their knobs.

This experiment further proves, that when two currents of the electric fluid, of contrary polar properties, get within the ſphere of each other's influence, they will ruſh together with a velocity ſufficient to light any very inflammable ſubſtance, at the place where the concuſſion happens. The ſucceſs of this experiment does not depend ſo much upon the ſtrength of the charge as the velocity of the ſpark; for I have often fired ſpirits of wine in this manner with four-ounce phials.

EXPERIMENT VIII.

To fire ſpirits of wine at ſix places with the ſame ſparks, and at the ſame time.

Slip out the bended wires H K, I K, with the two ſmall glaſs tubes F and G, from the large glaſs tubes Q, R, on the ſtand V; and put on upon each end of the two large tubes Q and R a ſmall metal plate S, fig. 3. plate II. which has a ſocket C to fit the ends of the tubes R or Q. Oppoſite to the ſocket, and at the other extremity of the plate, is fixed a ſocket to admit the upright glaſs tube D, which has a moveable ferrel round it E, to hold the horizontal ſocket G, which receives a ſmall glaſs tube H; and through it paſſes the bended wire K, to ſupply the place of the bended wire K H in the preceding experiments. As the bended wire K reaches no farther than the edge of the ſaucer at W, another ſhort glaſs tube with a wire muſt be put through the tube R, and the end of the wire H muſt only reach to the ſpirits in the ſaucer upon the plate C, upon the ſtand V. See the figure.

The additional part of the apparatus on the ſtand A is the ſame as above, only, inſtead of the curved wire K, uſe a ſtraight one between the coatings of the jars. When the apparatus is properly adjuſted, with a metal plate on the end of each tube, a ſaucer with ſpirits of wine upon each plate, at the ends and the middle of the ſtands, with the ends of the wires one in the ſpirits of wine, and the other upon the ſurface of the ſpirits; after the ſpirits have been warmed and lighted as before, charge the jars, and in diſcharging of them with the balanced rods W and X, the ſpirits of wine will be lighted at ſix places, by the paſſing of the ſparks between the knobs and the outſide coatings of the jars.

I have tried, and ſucceeded in a great many different experiments in lighting ſpirits of wine: I have alſo as great a variety of new experiments on electric attraction and repulſion; all of which tend to confirm me in my opinion of what I have already publiſhed, and now offer; *viz.* that glaſs is permeable to the electric effluvia, and that there is a polar virtue in the electric particles. If I have erred in my purſuit, I am open to conviction, and will thankfully receive any hint from the real friend of ſcience, which may direct me to truth. I will likewiſe attend to the candid animadverſions of the liberal critic; and I flatter myſelf I ſhall turn to ſome advantage the remarks of the zealous partizan for the Franklinian doctrine, who contents himſelf with walking in the path haſtily traced out for him, with an unphiloſophical indifference whether it will lead him to error or to truth.

To ſuch a perſon I ſhall only anſwer, that I have not accuſtomed myſelf to give up the evidence of my ſenſes to the errors of any name, however eminent or exalted; or to the prejudices of the votaries of ſcience, however numerous. I lay claim to that unalienable right of humanity, which impowers me to reaſon and to judge for myſelf; and I have not, like many, a predilection for a particular hypotheſis, any farther than as it appears to ſtand on the broad baſis of nature.

I ſhall only add, that if the Frankliniſts ſhould offer any thing more ſatisfactory than has already been publiſhed, to ſupport their favourite theory, I ſhall be ready to give it a fair and a candid examination; and if it ſhould carry conviction, I not only am ready, but will retract any miſtake which is publiſhed under my name.

F I N I S.

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